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THE

METHOD OF DARWIN

A Study in Scientific Method

BY

FRANK CRAMER

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TO

MY FRIEND AND TEACHER,

BRADFORD PAUL RAYMOND.

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PREFACE.

THIS attempt to analyze the method employed in the biological sciences arose from the belief that the direct study of scientific method, as it is illustrated by the works of the accepted masters, is worthy of far more careful attention than is usually accorded to it. As a rule, scientific men are so deeply engrossed in their investigations that they rarely undertake to discuss method. The logical processes involved and the nature of the difficulties met with in scientific investigation are the same as in the practical affairs of life. The fundamental processes of reasoning are the same everywhere; and it cannot but be helpful to study those processes as they are actually applied by master minds in fields where precision of method is peculiarly essential. Even though there may be grave question concerning the practical value of the study of formal logic, there can be no

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question concerning the importance of attention to the best practice in matters of reasoning.

Some of the reasons which induced me to select Darwin's works as a basis for an analysis of scientific method were: (1) the desire to confine the discussion to the writings of a single author, in order to concentrate the reader's attention upon a model; (2) the fact that his works cover a wide range of subjects, and can be read and understood by those who have had only a moderate amount of scientific training; and (3) above all, the fact that Darwin's investigations, and the reasoning based upon them, have furnished the biological sciences with their dominant principles.

To facilitate the study of his works from the point of view of method, references have been added to nearly all the examples drawn from them. A few examples have been mentioned or briefly discussed several times, and this may detract slightly from the freshness of some parts; but a partial compensation may be found in the fact that the repetition of the same example, under different divisions of the subject, will emphasize more strongly the complex inter-

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dependence of the various logical processes. In several instances, particularly those of radicles, climbing plants, and electric organs, the discussions have been carried into considerable detail: this has been done for the purpose of showing what an actual course of investigation and reasoning is like, — how results, whether true or false, are worked out.

At the same time, no effort has been made to make the treatment of Darwin's method exhaustive, nor has any formal explanation of the various logical processes been made. Those who are likely to read this book are already sufficiently familiar with the terminology of logic and the practice of science to understand it easily; and extended explanations would require an excursion into the domain of formal logic, which is not a part of the purpose of the present work.

Some of the most important processes have been selected, and Darwin's applications of them illustrated, in such a way as to confine the whole discussion within the narrowest possible limits. It is an easy matter to provoke differences of opinion in discussing either the

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nature or the names of the logical processes; but as far as it lay in my power I have avoided setting my foot on controversial ground. There are in the book, no doubt, many errors in detail, but it is hoped that no serious ones have crept in.

I wish to thank Professor Francis Darwin of Cambridge, England, and President David Starr Jordan of Stanford University, both of whom made important suggestions, and Professor H. B. Lathrop of Stanford University, who carefully revised the manuscript.

FRANK CRAMER.

MANZANITA HALL.
[Stanford University]

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LIST OF DARWIN'S WORKS REFERRED TO IN THE FOOT-NOTES.

Origin of Species, Sixth Edition, 1872.

Descent of Man and Selection in Relation to Sex, 1871.

Variation of Animals and Plants under Domestication, Second Edition, 1875.

Expression of the Emotions in Man and the Lower Animals, 1872.

A Monograph of the Cirripedia, Vol. I., 1851 ; Vol. II., 1854.

The Formation of Vegetable Mould through the Action of Earthworms, 1881.

Naturalist's Voyage around the World, 1860.

Geological Observations on Volcanic Islands and on Parts of South America, Second Edition, 1876.

Structure and Distribution of Coral Reefs, Third Edition, 1889.

Fertilization of Orchids, Second Edition, 1877.

Effects of Cross- and Self-Fertilization, 1876.

Different Forms of Flowers on Plants of the same Species, 1877.

Insectivorous Plants, 1875.

Climbing Plants, 1875.

Power of Movement in Plants, by Charles and Francis Darwin, 1881.

Life and Letters of Charles Darwin, by Francis Darwin, two vols., 1887.

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A SCIENTIFIC discovery is the outcome of an interesting process of evolution in the mind of its author. When we are able to detect the germs of thought in which such a discovery has originated, and to trace the successive stages of the reasoning by which the crude idea has developed into an epoch-making book, we have the materials for reconstructing an important chapter of scientific history. — PROF. J. W. JUDD, *Critical Introduction to Bettany's Edition of Darwin's "Structure and Distribution of Coral Reefs."*

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THE METHOD OF DARWIN.

I.

EDUCATION AND THE ART OF REASONING.

IT is an opinion not uncommon among educators that a definition of education which would cover all kinds of training is an impossibility. Such a definition, as it widens for the reception of manual training and the study of Greek, kindergarten work and the post-graduate course, certainly threatens to become "like a circle in the water, which never ceaseth to enlarge itself till by broad spreading it disperse to naught." The difficulty in framing it has apparently increased in recent years, since the old standards of value in education have had to struggle for existence with all the other college and university studies. The old definition, "to lead out and train the mental powers," is comprehensive enough for all purposes, for it tells neither what the mental powers to be trained are, nor how they are to be trained;

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by the change of a word or two it would apply equally well to the art of breaking colts. If the definition is stated more in detail, it is found to lie entirely in the domain of applied logic; and education of the intellect, in the only sense in which it can cover the whole field, is the process of training the intellect in the art of reasoning.

If there is or ever shall be a common aim in all phases of education, it will be based upon this common element. However important the information may be which is conveyed to the student in any department of study, his ability to retain it and use it for further acquisition depends entirely on the method by which he acquired it, and the degree to which he has become master of that method. The facts necessary for a new investigation are easily brought to hand if the intellect has been trained to work independently. One of the most striking things in Darwin's Autobiography is the relative importance, to be mentioned again hereafter, which he assigns, in his analysis of his own education,[1] to the accumulation of facts and to the development of mental habits. Probably few minds ever possessed in a higher

[1] Life and Letters of Charles Darwin, by Francis Darwin, Vol. I. pp. 51, 52.

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degree the power to collect and utilize facts. He said the real education of his mind began on the Beagle voyage. And yet he gave a very subordinate place to the vast number of facts with which he became acquainted on the voyage, and assigned supreme importance to the habits of incessant industry and concentrated attention which were developed in him, and to the necessity of reasoning in the solution of geological problems.

If the most important and only common element in education is the development of the power of reasoning, it may seem strange that logic, proudly called the science of sciences, should play so obscure a rôle that in many institutions it is practically ignored, and in the rest it is tolerated in a very brief course. The two most probable reasons for this are, first, the general notion that the human mind learns to reason as the human body learns to walk, that there is no need of teaching; that as training in the latter can only produce a Delsartean gait, which for practical purposes is little superior to an awkward wabble, so training in the art of reasoning is likely to produce nothing but over-refinement, which accepts indifferently postulates foolish and wise, and seeks only to draw out their consequences into

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gossamer threads. The second reason is, that the logic usually taught is not the logic of common life refined by successful scientific experience, but "formal logic," emptied of all contents and divested of all covering. Every fool can walk, and, as Darwin truly said, any fool can generalize and speculate. But the secret of originality, ingenuity, skill to seize facts, grasp their significance, and anticipate consequences, is not hidden here. Not the power to reason, but the power to reason quickly and unerringly and doggedly and impartially is the basis of success alike for the business man and for the man of science.

If skilful and accurate reasoning constitutes so essential an element of education, and if logic, as formally taught, is, for the mass of students, so barren of results, it is pertinent to inquire what provision for logical training is made in the general instruction of colleges and universities. In the evolution of the college curriculum the individuality of the student has finally been recognized and provided for, and the dignity of the sciences, as subjects conducive to mental discipline, has become an accepted fact. The material of education has by this been both increased and improved. Method has also undergone profound changes.

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The laboratory for science, sources of information for history, inventional work in mathematics, all bear witness that the student has been brought into direct contact with the material by means of which his intellect is to be trained. The best laboratory hand-books are no longer books of directions, but of suggestion and question; these books constitute a distinct recognition that the art of reasoning is the heart of education, that the true student is from first to last a discoverer, and that any method which makes the discoveries for him is wrong.

There are, however, two very distinct orders of reasoning: the order of discovery, which the mind follows as it winds its way among facts, adopting tentatively hypotheses which are afterwards rejected, and groping along the border of the unknown in the pursuit of knowledge; and the order of proof or argument, used by the investigator in his effort to convince his hearer or reader of the truth of his results. The order of proof may ignore entirely the steps by which the discoveries were made, the materials collected, and the conclusions drawn. The aim is conviction, and the evidence is arranged in the most lucid order to support the conclusions established at the end of an inves-

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tigation. It is true that sometimes the order of discovery is the best for purposes of proof; but unless it happen to be so, it is ignored after the investigation is completed.

Logicians insist that their science is the science of proof; that it does not furnish truth, but tests by which to determine whether or not a supposed discovery is truth. Books and lectures are invariably built up on the plan of proof. In them the question how a conclusion was reached is rarely presented, and when it does occur, pains is seldom taken to provide for its answer. So far, then, as these elements of education are concerned, the student is made a recipient. He is struck by the lucid arrangement of facts, the majestic sweep of the argument, and wonders why the world did not sooner get hold of truth that seems so conclusive to him. In the laboratory, the hand-books tell him what to look for and where to find it; and in the lecture-room the facts are arranged and the theoretical explanations are made for him. Thus in neither of the two practical divisions of the art of reasoning is he allowed to follow even the untrained impulses of his intellect. The average student knows next to nothing of the science of reasoning, and is largely prevented from practising the art of

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reasoning either from the standpoint of discovery or from that of proof.

This general indictment against the college curriculum needs to be hedged in by many qualifications; but there are none which seriously break its force. The graduate student is left largely to his own resources, and must do his own reasoning or go without any. A few of the best laboratory hand-books question and suggest to the student, so that he is kept from dissipating his energy; but they largely compel him to make his own discoveries and develop independence. A few teachers habitually, and many teachers occasionally, compel their students to cast their materials into the order of proof or argument in topical reports; but those very reports are, as a rule, the best illustration of the utter lack of logical insight on the part of students.

Before applied logic secures general recognition proportionate to its importance, it will have to demonstrate its ability to lay in the mind of the student the foundation of accurate thinking, to furnish him with analyses of models of successful reasoning, and criteria by which to detect false reasoning.

It is strange that while the study of our mother tongue and of all the sciences has

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undergone so great a revival, the science of reasoning should still be lying in the valley of dry bones. Fate may have decreed that its revival should be the crowning phase of modern progress in educational methods. Slowly but very surely these methods are drifting in the direction of a more extended and a more profound recognition of the importance of reasoning.

When educational practice shall have demonstrated the importance of the art of reasoning, scientific models of the art, for purposes of logical study, will be found to be rare, especially those that reveal the order of discovery. The scientist, after establishing a conclusion to his own satisfaction, is not concerned with telling other people how he reached it, but with convincing them of its truth. For this purpose he throws his conclusions and facts into the order best suited to form a compact argument. In the vast majority of cases it is impossible to follow out the original course of thought by a study of the results as they are embodied in a book. It would probably be difficult for an author himself to trace again the windings of his thought. Therefore, while there is a fair number of models for the study of argument, the writers who habitually take their readers so far into their confidence as to tell them by

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what steps they arrived at facts and conclusions, are extremely rare.

Several reasons have led to the following study of Darwin's method: first, conviction of the supreme practical importance of the direct study of scientific method; secondly, the fact that logicians and scientific philosophers draw their illustrations of scientific method almost exclusively from the physical sciences; thirdly, while those illustrations are fascinating on account of their brilliancy and their approach toward mathematical certainty, the biological sciences are much better adapted to furnish models for the average student, because in the nature of their logical difficulties they approach more nearly to the experiences of common life; fourthly, Darwin's custom of presenting all sides of a case very frequently led him to expose the original course of his thought and the order of his discoveries so clearly as to make the reader almost feel that he and Darwin are making the discovery together. Darwin consciously recognized or unconsciously felt that there was considerable power to produce conviction in an understanding of the particular way in which the truth was first reached. He so habitually took the reader into his confidence that he will probably always

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remain the clearest model in the biological world for the study of applied logic.

There are two ways open for the logical study of Darwin's works: one in which the methods of handling material and the different logical processes would be illustrated by examples from different parts of his works; and the other in which each of his investigations would be studied apart from the rest for the purpose of noting the part which induction, deduction, analogy, and verification play in producing the results. The former was chosen, because in that way the best examples of each of the logical processes could be brought together in the smallest compass, while the latter would require an elaborate summary of the works themselves before their logic could be discussed. No epitome, much less selected examples, can be made a substitute for a logical study of the works themselves; it can at most serve as a guide to further study.

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II.

DARWIN'S VIEWS OF METHOD.

IT is necessary to inquire briefly into Darwin's own views of scientific method. He has given us the data for the inquiry, both in direct statements and indirectly by his opinions of the work and ability of other men. In connection with this inquiry must be considered the intellectual and moral qualities of the man himself, and the external influences that bore upon him.

In some quarters the notion is entertained that the scientific method leads infallibly to the truth, and that it is something quite distinct from the logical method of every-day life; and yet there is a general haziness as to what the scientific method is. The aim of the scientist is truth, but he has no special mental faculty with which to discover scientific truth. He approaches his problem, equipped with the same logical processes that the common man uses in arriving at facts that are important to his success in life. Neglect in applying those

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processes strictly, in either case, results in failure, and fidelity to them is the measure of success. There is something more than merely a weak analogy between the very small proportion of successful business men and the similarly small proportion of really successful creative scientific men. The failures on both sides are due to the same kind of intellectual errors. Nor are the principles of the scientific method less clearly understood by successful business men than by successful scientific investigators.

Darwin has said that the training which he got on the Beagle voyage was the first real education of his mind.[1] His University course at Cambridge had been mechanical. He declared that the study of Paley's "Evidences" and "Natural Theology" gave him as much delight as did Euclid.[2] And he believed at the time, and to the end of his life, that the study of these works was the only part of his academic course that contributed to the education of his mind. He got no inspiration, and very little knowledge, from his medical course at Edinburgh; one of the principal records of that course is his opinion that there are no

[1] Life and Letters, Vol. I. pp. 51, 52.

[2] Ibid., p. 41.

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advantages and many disadvantages in lectures compared with reading.[1]

He has given in his Autobiography the various special subjects that occupied his attention on the Beagle voyage. He attended to Zoology, Botany, and Geology. After mentioning the others, he said that Geology "was far more important, as reasoning here comes into play. On first examining a new district, nothing can appear more hopeless than the chaos of rocks; but by recording the stratification and nature of the rocks and fossils at many points, always reasoning and predicting what will be found elsewhere, light soon begins to dawn on the district, and the structure of the whole becomes more or less intelligible."[2] It is interesting to recall that he finally committed the shooting of birds to his servant in order that he might devote himself to the geology of the districts in which he worked. The zoological and botanical materials which he collected were largely worked up by other scientists after his return. On these subjects he collected a vast amount of information, which gave birth to his great biological theories, and was indispensable in the work of his later life. But he

[1] Life and Letters, Vol. I. p. 33.

[2] Ibid., pp. 51, 52.

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did not work this information into a system and bend his energies upon it. He devoted himself to the solution of geological problems in the field; while the biological problems only arose in their early shadowy outlines during the voyage, and remained in the form of questions till long after his return. Thus it came about that from an educational point of view his biological work was secondary and the geological work pre-eminent. Had his life-work ended with the reports of the Beagle voyage, he would have been rated as a geologist. But after his return he exercised upon great biological problems the intellectual strength and vigor which had been developed by the solution of geological problems in the field. "The above various special studies were, however, of no importance," he said, "compared with the habit of energetic industry and of concentrated attention to whatever I was engaged in which I then acquired. Everything about which I thought or read was made to bear directly on what I had seen or was likely to see." He had problems constantly before him, and the time allowed for their solution was always limited by his own movements and those of the ship; so that energy and concentration became habitual in a mind already

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strong by nature. His isolation from other scientific men and from books doubtless also developed in him the habit of using all the facts that presented themselves, and directly and indirectly getting at their significance. He could not lay his hands on ready-made explanations of the facts that came before him, and was compelled to explain them himself. He said of himself, "I think I am superior to the common run of men in noticing things which easily escape attention, and in observing them carefully. . . . From my earliest youth I have had the strongest desire to understand or explain whatever I observed, — that is, to group all facts under some general laws."[1] These natural traits were of necessity strengthened and developed by their incessant exercise on the voyage.

Another prominent trait in Darwin was the accuracy with which he made his observations and experiments. "He saved a great deal of time through not having to do things twice." And he always "wished to learn as much as possible from an experiment, so that he did not confine himself to observing the single point to which the experiment was directed, and his power of seeing a number of things

[1] Life and Letters, Vol. I. p. 83.

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was wonderful."[1] Both his accuracy of observation and his grasp of *all* the facts connected with an experiment were doubtless made habitual on the voyage by the never absent consciousness that there was only one opportunity to do whatever he did.

During that memorable voyage Darwin's education went on, unhampered by laboratory hand-books with directions for finding the facts, or by professors to do the reasoning for him either before or after the facts were found. In all his work there was the complex and incessant interplay of observation, induction, deduction, and verification which constitutes the scientific method. The necessity of working rapidly, accurately, and thoroughly forced upon him by the consciousness that his time was limited, and that the work could not be done over again, coupled with his native energy and independence, accounts for the character and quantity of his scientific work.

The mental traits alluded to were coupled with remarkable conscientiousness. In the long run it pays the scientist to be honest, not only by not making false statements, but by giving full expression to facts that are opposed to his views. Moral slovenliness is visited

[1] Life and Letters, Vol. I. pp. 121, 122.

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with far severer penalties in the scientific than in the business world. Scientific results are used as foundations for further investigations, and for this reason they are tested again and again; and if any man's work is unreliable it is done over by some one else, who reaps the permanent credit. But the temptations to make statements too broad, to neglect objections, to smooth over difficulties superficially, are almost infinite. There is apparent throughout all of Darwin's work much more than the intellectual uprightness that is due to a belief in "reward and punishment." The very grain of his scientific character was conscientiousness.

His educational history, his thoroughness, his scientific honesty, his logical power, his power of minute observation and broad generalization, the greatness of the problems with which he dealt, and the profound influence of his views upon the thought of the world, all conspire to make him a model in the study of scientific method. Some of his views have been rejected, and many may be profoundly modified by more accurate knowledge, but these things will in no way affect the value of Darwin as a type of what education should accomplish, and how it must accomplish it.

Darwin appreciated the humblest scientific

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work, and listened with deference to the suggestions of others. He never dealt out primitive justice to his opponents on the principle of an eye for an eye and a tooth for a tooth. He is morally famous for the forbearance that he exercised toward those who attacked him. This fame is heightened by the fact that he was an acute judge of mental quality. His investigations made it necessary for him to collect information from all sorts of sources, — not only at first hand, from Nature herself, but at second hand, from many kinds of books, made by many kinds of men. He has complained that it was exceedingly difficult to find out what and whom to trust.[1] But his criticisms of certain kinds of work show how definite were his standards of value in measuring scientific results.

Perhaps the most savage things Darwin ever wrote are contained in letters to Hugh Strickland, and relate to the nomenclature of systematic zoology and botany.[2] He expressed freely his contempt for describers of species who think the honor consists in having one's name appended to that of a newly described species, and whose work is generally so inac-

[1] Life and Letters, Vol. II. p. 75.

[2] Ibid., Vol. I. pp. 334, 338, 344.

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curate or imperfect, or both, as to be practically worthless for any of the higher purposes of science. Nor was he satisfied with mere details even when they were accurate. In his reminiscences of Robert Brown he said that Brown seemed to him "to be chiefly remarkable for the minuteness of his observations, and their perfect accuracy."[1] Darwin often took breakfast with Brown, and on those occasions the latter, according to Darwin, "poured forth a rich treasure of curious observations and acute remarks; but they almost always related to minute points, and he never with me discussed large or general questions of science."

He has given us an interesting example of his opinion of the opposite tendency toward speculation, to the neglect of facts. During his career as a medical student he admired greatly his grandfather Erasmus Darwin's "Zoonomia."[2] "But on reading it a second time," he said, "after an interval of ten or fifteen years, I was much disappointed, the proportion of speculation being so large to the facts given." These criticisms of the work and methods of others are in perfect accord

[1] Life and Letters, Vol. I. pp. 57, 60.

[2] Ibid., p. 34.

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with his own practice. He combined in himself the qualities of both Brown and his own grandfather. His works are a series of models of the scientific method, because of the rare and happy combination of minute and accurate observation and daring speculation followed by ruthless testing and pruning of his hypotheses. He thought it worth while to notice and penetrate into the meaning of the most insignificant fact, and was capable of sweeping the whole earth for evidence in support of his largest theories. He could take the time to count twenty thousand seeds of *Lythrum salicaria;*[1] and his prophetic philosophical eye led him to exclaim, "What a science Natural History will be when we are in our graves, when all the laws of change are thought one of the most important parts of Natural History!"[2]

At various times and under various circumstances Darwin expressed fragmentary opinions concerning what constitutes scientific method, but never tried to make a complete statement of it. His notion of what the method is, is shown mostly by what he said concerning "deduction." For instance, concerning Bastian's work, he said, in a letter to Wallace, "I am

[1] Different Forms of Flowers, etc., p. 189.

[2] Life and Letters, Vol. I. p. 439.

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not convinced, partly, I think, owing to the deductive cast of much of his reasoning; and I know not why, but I never feel convinced by deduction, even in the case of H. Spencer's writings";[1] and in a letter to John Fiske, "I find that my mind is so fixed by the inductive method, that I cannot appreciate deductive reasoning; I must begin with a good body of facts, and not from principle (in which I always suspect some fallacy), and then as much deduction as you please."[2]

Now deduction means, in one of its senses, reasoning from the general to the particular, from a law, principle, or general fact to a particular fact. But in the above quotations Darwin meant by deduction, and the deductive method, reasoning from postulates the truth of which is accepted as *beyond dispute.* Induction, as a logical process, means reasoning from particular to general, from facts to laws or principles. But induction, or inductive method, when used in a sense synonymous with scientific method, includes all the logical processes, induction, deduction, analogy, verification, — every way in which the intellect passes from fact to fact. This is widely different from what Bacon originally meant by induc-

[1] Life and Letters, Vol. II. p. 346. [2] Ibid., p. 371.

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tive method; but practically no scientific man has ever followed Bacon's method.

The inductive method, as illustrated by Darwin's own work, and as understood by all who think clearly on the subject, consists in the formation of an hypothesis from the facts by induction at the earliest possible moment in an investigation, deductive application of the hypothesis to known facts, and in the search for others that ought to exist if it is true, until it proves itself imperfect. By the help of the new facts the hypothesis is improved (by induction) and again applied, until by successive approximations it reaches the truth. So that in the so-called inductive or scientific method deduction is far more extensively used than induction. But to say that one of the processes is more important than the other would be like saying that the female element, for example, is more important for reproduction than the male element. It is interesting to note in this connection that John Stuart Mill, the modern logician who has stood out as the champion of the inductive method, has inconsistently described the combination "hypothesis, deduction, and verification," as the deductive method.[1]

[1] Mill, System of Logic, People's Edition, p. 304.

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The scientific or inductive method as understood and practised by Darwin begins and ends with facts. It takes nothing for granted that relates to the matter under investigation, and assumes as true only such things as the law of causation and the validity of the reasoning processes; while the deductive method, as understood by him, starts from principles whose truth is not questioned. Regarded simply as a logical process, however, deduction is equally valid whether the premises are assumed to be true or admitted to be hypothetical.

The ideal attitude of the scientific mind is beautifully described in Darwin's own words concerning himself: "I have steadily endeavored to keep my mind free so as to give up any hypothesis, however much beloved, (and I cannot resist forming one on every subject,) as soon as facts are shown to be opposed to it. Indeed, I have had no choice but to act in this manner, for, with the exception of the Coral Reefs, I cannot remember a single first-formed hypothesis which had not after a time to be given up or greatly modified. This has naturally led me to distrust greatly deductive reasoning in the mixed sciences."[1]

[1] Life and Letters, Vol. I. p. 83.

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It has been said that deduction, regarded merely as a logical process, is equally valid whether the premises are assumed to be true or are admitted to be hypothetical. If the premises are true, the conclusion *must* be true. What brought the old deductive process into so general disrepute was not its inadequacy, but the using as premises in the process principles that were held to be beyond dispute. To this end the *a priori* reasoner went farther and farther back in his philosophical repertory, until he reached principles or axioms that he felt could not be denied. Then by an irresistible deductive swoop he reached conclusions that must likewise be true. In such philosophy there is little need of verification.

In science, and therefore also in the reasoning of practical life, the great question always is whether the premises are true, or partly true, or false. The old notion was that deduction led to certainty, and induction did not. But in the scientific method the object is not merely to deduce consequences from laws or principles, but to establish the truth or falsity of those laws or principles themselves. Hence there is an incessant interplay of induction and deduction. Darwin's distrust of deductive reasoning was due to his fear of the premises.

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But he said that all his hypotheses had to be abandoned or modified, and they were the conclusions of inductions; so that inductive reasoning by itself is also absolutely worthless. The truth is, Darwin trusted nothing. Induction furnished him hypotheses, and deduction interpreted known facts and led to new ones under those hypotheses; but verification of his deductions was as indispensable to him as sunshine to a plant.

Darwin himself said, "Any fool can generalize and speculate." There has always been an over-abundance of reasoning, both inductive and deductive. Untrammeled induction is largely responsible for the wild and worthless beliefs that have burdened the world; and untrammeled deduction is as largely responsible for the dogmatic dry-rot that has prevented progress in human discovery and beliefs. Darwin was one of the most powerful deductive reasoners that ever lived; and he was perfectly fearless in making inductions, for, as he said himself, he made an hypothesis on every subject. But the illustrations of the various logical processes that have been drawn from his works show that even he — with his almost superhuman powers of observation, his innate desire to refer every fact to a general law, his

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rare ability to reason out the consequences of his hypotheses, and his unbending determination to test his reasoning by ruthless investigation — made important inductions, deductions, and analogies that were not true, and failed to make deductions that should have thrust themselves upon him. He rarely, however, fell into the old and vicious error of thinking that reasoning of any kind is final *proof*.

He built up a large inductive structure in pangenesis only to see it rejected; he went wrong in his deduction concerning the relation between high degree of specialization and the chances in favor of preservation of a species; and was prevented by a bad analogy from investigating the effects of cross- and self-fertilization in plants until the subject was thrust upon him by empirical observation. He was as productive of hypotheses as Nature is of living things, and, like her, he subjected them all to the principle of natural selection. His mind was so fertile in guesses and so quick in testing them that he called much of his work "fool's experiments." But in this way nothing escaped him.

In recent years there has been made a formal statement of the reasons why fertility in hypotheses should be cultivated, and how they

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should be used.[1] The principle of multiple hypotheses is urged, because in the sciences it is not generally possible to hit at once upon a cause from a study of the facts. As many hypotheses as possible should be invented to explain the facts under investigation, and as fast as possible, as new light comes, other hypotheses should be added, in order that the mind may, as far as possible, put itself in possession of all the possible causes of the phenomena. By keeping them all constantly before it, it can consider every fact from many points of view; and each hypothesis will furnish its own clews to further evidence. In this way, also, the mind can more easily keep itself in a judicial attitude. With the increase of knowledge some of the hypotheses are shown to be inadequate, and by the process of exclusion their number is reduced until the investigation ends in the establishment of the true theory of the cause of the facts under investigation.

In actual practice a good many difficulties are connected with this method of using hypotheses. Among a number of hypotheses one will almost invariably have a somewhat higher degree of probability than the rest; the

[1] T. C. Chamberlain, in Science, Feb. 7, 1890.

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mind, working along the lines of least resistance, follows the clews it offers to the neglect of the other hypotheses. The more usual practice is typified in Darwin's method. He made an hypothesis at the earliest possible moment, and began to work with it. With increasing knowledge it was modified, or rejected and replaced by a more likely one. So that there was a succession of hypotheses or of improvements of the original one.

The method of multiple hypotheses is common enough, if it be made to include not only the instances in which the same individual entertains several hypotheses, but also those in which different hypotheses are entertained by different people. There are few questions on which there are not several opinions; and one approaching the subject impartially considers them together in order to adopt the most likely one. The process of exclusion works *admirably*, and the result amounts to demonstration when all the possible hypotheses are *known*, and one needs only to show that one of them agrees with the facts and the others do not. Newton, in establishing the law of gravitation, showed that the orbits and velocities of the planets would be what they are if the attractive force resided at the centre of the

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system or in the sun, and acted with a force varying inversely as the square of the distance; and that they could *not* be what they are if the force were located anywhere except at the centre. In this instance Newton was able to exclude mathematically every theory except the true one; and the demonstration was made complete by the positive proof that the facts accorded with the one hypothesis and were at variance with every other.

Darwin used the principle of exclusion in one of his early scientific efforts. There were two hypotheses to account for the existence of the great terraces called benches, or parallel roads, of Glen Roy, in Scotland. It was evident enough that they had been formed by the action of water; and they must either have been formed by the sea, in which case the uppermost, for example, must have been afterwards elevated more than one thousand feet; or they must have been the ancient shores of a lake formed by the blocking up of the open side of the now empty lake bed. Darwin studied the region, and concluded that the benches were of marine origin. He could not conceive them to be due to barriers of rock or detritus. He was then fresh from his studies on the geology of South America. On the

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coasts of that continent he had grown familiar with the immense scale on which elevations take place, and had many opportunities to study marine sand and gravel formations that are now hundreds of feet above sea level. It was therefore as easy for him to conceive a marine origin of the terraces of Glen Roy as it was difficult for him to believe that they were the old shores of an elevated lake that had been blocked in. He adopted the method of exclusion, proved the one hypothesis probable and the other improbable.

But this instance of the principle of exclusion illustrates well the danger to which the investigator is exposed in its use. In geology, zoology, etc., — in what Darwin calls the mixed sciences, — one can rarely know whether all the possible hypotheses are known. Newton could make a rigid demonstration, not simply because he could treat his problem mathematically, but because he was able to treat all the possible hypotheses. When the glacial theory was introduced into the geological world the ancient terraces high above sea level in the colder temperate zone were accounted for. Then it was plain that the terraces of Glen Roy had been the shores, not of the sea, nor of a lake dammed up by rock

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or detritus, but of a lake dammed up by glacial ice.

Darwin was ashamed of his arguments and conclusions about Glen Roy. "My error," he said, "has been a good lesson to me never to trust in science to the principle of exclusion."[1] But it is inevitable that apparently definite views should receive just such shocks upon the introduction of a great new principle in a science. The facts that find their explanation under the single newly discovered cause are necessarily referred, before the advent of the new hypothesis, to very various and unrelated causes.

On the question of the origin of species there were really two hypotheses, creation and descent, when Darwin took hold of it; and he adopted the process of exclusion in treating them. The evidence was all in favor of descent by natural selection and opposed to creation. But he was himself emphatic in the declaration that the origin of species by natural selection was not demonstrated. Belief in it must be based on general considerations, — that natural selection is an actually existing cause, and that it explains a host of facts and brings them under one point of view. One hypothesis

[1] Life and Letters, Vol. I. p. 57.

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was excluded and the other adopted. And the one that was accepted was based, not on direct proof, but on one of the most magnificent series of deductions that the world has ever seen.

The further discussion of this subject will be deferred for the present. In the following chapters Darwin's method of treating the problems that presented themselves to his mind will be analyzed in some detail, and the closing chapter will deal with the logical history of the principle of natural selection.

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III.

STARTING POINTS. — METHOD OF STARTING INVESTIGATIONS. — ISOLATED PHENOMENA.

THE starting points of many of Darwin's researches were furnished him by other intelligent men. In many cases these men not only were in possession of the facts, but had hit upon their true explanation. With the facts thrust upon them, with enough reasoning ability to pursue them, they gave away their heritage, — luckily to one who knew its value. In his frank but modest analysis of his own mental qualities he said of himself, as already quoted, "I think I am superior to the common run of men in noticing things which easily escape attention, and in observing them carefully. . . . From my earliest youth I have had the strongest desire to understand or explain whatever I observed, . . . that is, to group all facts under some general laws."[1] There can be no doubt that his great interest in apparently little things, and his efforts to make

[1] Life and Letters, Vol. I. p. 83.

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the most of them, were due to his conviction that important things were hidden behind them, that they were illustrations of general laws.

Lawson, Vice-Governor of the Galapagos Islands at the time of Darwin's visit, knew that the tortoises of the different islands differed from one another, and even declared to Darwin that he could tell from which island any tortoise came.[1] He had the time, and the material lay at his feet; but he left it for Darwin to make the Galapagos Islands famous as illustrations of the local variations of species. Darwin himself had to have the evidence thrust upon him from several directions before he grasped its significance, but his greater appreciation of the nature and value of the facts made him their master.

After his return from the Beagle voyage, Mr. Wedgwood of Maer Hall suggested to him that the apparent sinking of superficial bodies, ashes, marl, cinders, etc., into the earth is due to the action of earth-worms.[2] Both the facts and the theory were ready at hand. To the one man they were probably interesting as intellectual playthings; to the other they became the starting point for a

[1] Naturalist's Voyage around the World, pp. 393–398.

[2] The Formation of Vegetable Mould, etc., p. 3.

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long investigation. Darwin read one of his first papers, "On the Formation of Vegetable Mould," before the Geological Society of London, November 1, 1837; and on the same apparently insignificant subject he published the last book of his life.

While collecting in the Chonos Archipelago, Southern Chile, he found numbers of an insignificant little cirriped, none more than one tenth of an inch long, embedded in the shell of *Concholepas peruviana.*[1] The zoological material of his trip was turned over to specialists for description, he furnishing the field notes and editing the publications. Much zoological information was thus given to the world, but none of all that material ever served as a starting point for a great investigation. The little abnormal cirriped was left to Darwin himself; probably because it was too small an affair to be taken charge of by others. In his hands it became the germ of a monograph on the Cirripedia, which is still the classical literature of the group. To determine its position he studied the structure of as many genera as possible. Dr. J. E. Gray, who had already collected a large amount of material for a mon-

[1] A Monograph of the Cirripedia, Vol. I., Preface, p. 5; Vol. II. pp. 566–586.

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ograph of the group, turned it over to Darwin. Gray did many things, but none well enough to make what he wrote indispensable in the study of any subject; he will be remembered chiefly as a keeper of the zoological department of the British Museum, and as a bitter opponent of Darwin's theory of descent, while the latter's monograph heads the list of cirriped literature.

Boitard and Corbié merely made the observation that, when they crossed certain breeds of pigeons, birds colored like the *Columba livia*, or common dove-cot, were almost invariably produced.[1] It drew Darwin's attention and led to numerous experiments on reversion due to crossing. Certainly some, perhaps many, scientific men had known that the species of sundew (*Drosera*) catch insects. Darwin himself had heard as much. Exhausted by his long labors on the Origin of Species, he was resting near Hartfield during the summer of 1860, and "was surprised by finding how large a number of insects were caught by the leaves of the common sundew (*Drosera rotundifolia*) on a heath in Sussex."[2] The right mind

[1] The Variation of Animals and Plants under Domestication, Vol. II. p. 14.

[2] Life and Letters, Vol. I. p. 77.

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had been impressed, and away the giant intellect started on another long and weary, but successful, search after truth. Mr. W. Marshall knew that in the mountains of Cumberland many insects adhered to the leaves of *Pinguicula;* he told Darwin, and Darwin told the world.[1] Mr. Holland's statement that water insects are often found imprisoned in the bladders of *Utricularia* is interesting, chiefly because it led Darwin to investigate the genus.[2]

The most fertile suggestions, however, came to him from the facts brought out by his own work. He did not record the hundredth part of the tentative notions that entered his mind; but many of the most important and lasting had their rise in what to most other men is the refuse heap of curious and exceptional little facts. Perhaps one of the noblest lessons he left to the world is this, — which to him amounted to a profound, almost religious conviction, — that every fact in nature, no matter how insignificant, every stripe of color, every tint of flowers, the length of an orchid's nectary, unusual height in a plant, all the infinite variety of apparently insignificant things, is

[1] Insectivorous Plants, p. 369.

[2] Ibid., p. 395.

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full of significance. For him it was a historical record, the revelation of a cause, the lurking place of a principle.

A typical example of his treatment of little exceptional facts is that of "Hero," the unusual plant in one of the later generations of self-fertilized plants of *Ipomœa purpurea*.[1] It was a little larger than the crossed seedlings of the same generation, and the first exception that had arisen, in his many experiments, to the rule that the crossed are superior to the self-fertilized seedlings in size and vigor. It was a little thing, even for an exception, and had occurred only after a very long series had established the rule. It was very fit to suffer the common fate of exceptions, and to be deliberately choked for the benefit of the rule to which it was so inconveniently related. By other hands it would probably have been recorded and then ignored. Darwin made it the parent of a whole race of exceptions. He found "Hero" to be exceptional, not only in being unusually tall, but in its being highly self-fertile, in its great powers of growth, and in its descendants which were crossed having no advantage over its self-fertilized descendants.

His aim, as soon as he hit upon a line of

[1] Effects of Cross- and Self-Fertilization, p. 60.

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investigation, was to reach as quickly as possible a crucial test or a crucial observation that would enable him to determine positively whether or not his beliefs were justified. As soon as the idea of descent of species took definite shape in his mind, he determined, after deliberation, to take up the study of domestic pigeons.[1] He selected these because the variations were more numerous and plainer, more of them had arisen in the historical period than is usual with animal groups, the material was abundant and easily accessible, etc. He chose for his investigation the conditions most favorable to success.

When he discovered that insects are caught in large numbers by the common sundew, he gathered a number of plants, counted the leaves and the number that had caught insects; and compared the results with the accidental destruction of insects by the viscous buds of the horse-chestnut, etc.[2] Similar results were produced by somewhat similar means; but by the comparison he secured preliminary evidence that the leaves of the sundew, unlike the buds of the horse-chestnut, were excellently *adapted*

[1] Origin of Species, p. 15; Variation of Animals and Plants under Domestication, Vol. I. p. 137.

[2] Insectivorous Plants, pp. 1–3, 63–76.

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for catching insects. To him adaptation for insect catching meant that this habit was advantageous to the plant; and that it must derive nourishment from the captured insects. He knew what elements plants required for nourishment, and immediately set about making another crucial test. When led to believe that the leaves absorbed nutritious matter from the insects, he made a crucial experiment by immersing numbers of the leaves in nitrogenous and non-nitrogenous fluids of the same specific gravity to find whether they would act differently in the two cases, taking care that the two sets of conditions should be the same except in the presence of nitrogenous matter in the one and its absence in the other. The test confirmed his belief, and is an example of the most rigid type of reasoning and experiment, — a combination of positive and negative evidence which Mill called the Method of Difference. Darwin's clear notion of what constitutes good evidence led him to seek demonstrative evidence by the shortest way. In the case just described he secured it at once, and might have rested content with having established the principle; but as soon as he found that the nitrogenous fluid alone excited energetic movements "it was obvious

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that here was a fine new field for investigation." His crucial tests only gave him confidence that there was more beyond; then he began the long series of observations and experiments which resulted in the charming volume on "Insectivorous Plants."

It is told elsewhere how he was deterred by theoretical considerations from experimenting on the effects of cross- and self-fertilization, and how the expectation of early results was fairly thrust upon him by the difference in size and vigor between crossed and self-fertilized seedlings.[1]

When once his attention was fixed, he made a preliminary experiment on two plants, with the effects of cross- and self-fertilization as the principal object of investigation. The results corroborated his previous observations, and he was in possession of the principle. Such simple preliminary experiments are interesting, since, if they do not establish a principle fully, they raise up for it a higher degree of probability than any succeeding experiments, and make it possible to work deductively with considerable confidence.

The introduction of the principle of continuity into general scientific thinking has

[1] Effects of Cross- and Self-Fertilization, pp. 6–8.

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made it a normal intellectual process to look upon exceptional and isolated phenomena as merely extreme instances of much larger groups. One of the marked characteristics of Darwin's work is that he selected such extreme instances, and sought to connect them with the more common facts to which they were related, by proving, or at least suggesting, their derivation from the latter. Wherever it was possible in his experiments, he varied the amount of a cause in order to note the proportionate variation in the amount of the effect; and where he had to depend upon observation alone, he made strenuous efforts to connect extreme instances by gradations of character.

Thus, he and his son Francis, by *continuous* attention to the sleeping movements of plants, were able to show that it is not true, as is generally supposed, that the leaves move only in the evening when going to sleep, and in the morning when awaking; for they found no exception to the rule that leaves which sleep continue to move during the whole twenty-four hours, only moving more quickly when going to sleep and awaking than at other times.[1] They were able to show that sleeping movements are only highly specialized and exaggerated modifica-

[1] Power of Movement in Plants, p. 403.

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tions of the universal movement of circumnutation. In his experiments on insectivorous plants with phosphate of ammonia he varied the proportion of the latter to determine how small an amount would affect the tentacles of Drosera.[1] He found that excessively minute quantities of the latter would produce reaction. His results were so astonishing that in 1873 he doubted his own experiments of 1872, and in 1874 he again thought that some mistake must have been made, and again repeated the experiments, but always with the same results. He discussed these remarkable facts at some length, tried to make them more credible by comparing them with similar cases that are equally astonishing but are known to be true; and expressed the hope that his experiments would be repeated, at the same time laying down the conditions of success.

While studying the power of circumnutation in plants the Darwins accidentally left some of their specimens in several cases exposed to oblique light. Before they "knew how greatly ordinary circumnutation was modified by a lateral light, some seedling oats, with rather old and therefore not highly sensitive cotyledons, were placed in front of a northwest win-

[1] Insectivorous Plants, pp. 154–173.

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dow, towards which they bent all day in a strongly zigzag course. On the following day they continued to bend in the same direction, but zigzagged much less. The sky, however, became, between 12:40 and 2:35 P.M., overcast with extraordinarily dark thunder-clouds, and it was interesting to note how plainly the cotyledons circumnutated during this interval."[1] These observations they considered of some value from their having made them while they were not attending to heliotropism; and they were led by them "to experiment on several kinds of seedlings, by exposing them to a dim lateral light, so as to observe the gradations between ordinary circumnutation and heliotropism." An accidental observation led to variations in the experiments, which resulted in demonstrating continuity between two apparently distinct classes of movements.

In some of his remarkable studies on gradations of characters, where it was impossible to make experiments, he sought out and observed Nature's own variations. Perhaps the most striking instance of the study of gradations of character is that connected with the "ocellus" on the tail coverts of the peacock.[2] This

[1] Power of Movement in Plants, p. 421.

[2] Descent of Man, Vol. II. pp. 132–145

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feather-mark was properly considered a serious difficulty to Darwin's theory because of its remarkable character. But with consummate ingenuity he undertook to connect it by a series of less and less remarkable markings with the ordinary feather-markings of the group to which the peacock belongs. It is impossible to exaggerate the importance of studying phenomena in their quantitative and qualitative variations, for on it depends the establishment of continuity between phenomena apparently widely separated, and it frequently leads to results that can be reached in no other way.

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IV.

EXHAUSTIVENESS.—TIME GIVEN TO INVESTIGATIONS.—TREATMENT OF OBJECTIONS.

THE theories with which Darwin dealt were so general, and the facts that had to be handled as evidence were so vast in number, that probably no man was ever exposed to greater temptation to collect his evidence promiscuously from all quarters, picking up in each field what was already known, and supplementing it by a few test observations. But he never contented himself with sketching theories and adorning them with dashes of evidence.

He made himself invincible by the exhaustiveness with which he determined the quality of his evidence. The great confidence which scientific men have had in him has been due to the fact that he did not leave it to them to test the theories which he presented. He convinced the world of the truth of a doctrine which others had striven in vain for fifty years to establish. To my mind one of the chief

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characteristic differences between his work and that of Lamarck and others, apart from differences in the explanations offered, is its superb exhaustiveness. It is as impossible now to take the ideas of descent and of natural selection out of the world as to take a star out of the sky. The firm establishment of these ideas was due to the quality and quantity of Darwin's work, and both of these were determined by the same exhaustiveness in method.

When he started out to describe the single little abnormal cirriped from the west coast of South America, he was characteristically led, as he said, for the sake of comparison, to examine the internal parts of as many genera as he could procure. This untamed determination to find out all there was to know about what he was describing was associated with a fine contempt for the kind of work that merely describes new things without showing all their connections. One of the greatest and most constant obstacles to his progress was that this intellectual quality was so rare or so little cultivated in other naturalists; so much of the scientific material with which he had to deal was so superficially or carelessly worked out that he never knew what to trust.

Among the best examples of this spirit of

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exhaustiveness is his study of pigeons.[1] As usual, he knew clearly what he was after, and this gave him the power of selecting judiciously the lines along which to make investigations and of using to the best advantage the materials he worked on. In his remarks on the search for the cause of the modification of species, he said, "Believing that it is always best to study some special group, I have, after deliberation, taken up domestic pigeons." With other ends in view, pigeons might be studied in different ways. But remembering his purpose, his work on pigeons is a model of exhaustiveness as well as of reasoning. He not only studied the variation of breeds, but sought its explanation by a minute study of individual differences. He considered the skeleton as well as the feathers, and gathered facts and specimens from all over the world.

What is true of his study of pigeons is true of his work on orchids. The adaptation of flowers for cross-fertilization had interested him for many years, and he had collected a large mass of observations; but he was true to his instinct: "It seemed to me a better plan to work out one group of plants as carefully as I

[1] Variation of Animals and Plants under Domestication, Chaps. V. and VI., pp. 137–235.

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could rather than publish many miscellaneous and imperfect observations." Orchids furnished an extreme case; and his work on them is fascinating from the nature of the subject, the end aimed at, and the ingenuity of the reasoning employed. He showed "how admirably these plants are constructed so as to permit of, or to favor, or to necessitate cross-fertilization"; but the way in which he did it is as admirable from a logical point of view as the flowers themselves are in their peculiar adaptation. By thus selecting judiciously the most extreme special cases for exhaustive examination, he threw the strongest light on all the collateral evidence, and made it easy for him to understand its significance. On the shoulders of such work his theories sat firmly, and it made it easy for those who came after him to work out the classes of facts which he was not able to exhaust.

In each of the many special studies which he carried on there are many models of method in the pursuit of details. The following case is especially interesting because it illustrates both the habitual care of the authors in their experiments on the movements of plants and the extreme liability to error that results from a wrong start. Since the book on the "Power of

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Movement in Plants" was written, it has been shown that the conclusion of the authors that "an object which yields with the greatest ease will deflect a radicle" is wrong. The whole superstructure of reasoning which resulted in the notion that the tips of radicles are sensitive to contact was therefore built on sand. The experiments and reasoning will be discussed from the point of view of the authors at the time they were made, and afterwards attention will be called to the corrections that have since been made by others. In their work on the movements of radicles, Charles and Francis Darwin found that "an object which yields with the greatest ease will deflect a radicle." Extremely thin tin-foil on soft sand was not at all impressed, and deflected the root at right angles. Hence, they reasoned, the cause of the deflection could not be mechanical contact. A conceivable hypothesis was that "the gentlest pressure might arrest growth and the apex grow only on one side; but this view leaves unexplained the curvature of the upper part, extending for a length of 8–10 mm. . . . We were therefore led to suspect that the apex was *sensitive to contact*, and that an effect was transmitted from it to the upper part."[1] By the

[1] Power of Movement in Plants, pp. 131–140.

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exclusion of the other two hypotheses — mechanical contact and arrest of growth — they confined themselves to the last one, sensitiveness to contact. This would have been a fine field for a discussion of the known facts followed by a necessary inference. Of the three possible hypotheses, two had been excluded, and the third must be true. It would seem quite clear that the case was logically proved. But instead of making this the end, they made it the beginning of their work.

They "thought that any small hard object affixed to the tip of a radicle freely suspended and growing in damp air, might cause it to bend if it were sensitive, and yet would not offer any mechanical resistance to its growth." The results of their experiments proved remarkable. When approaching the subject they made a preliminary trial with seven beans at a rather cool temperature, and six radicles curved. To quote again, "These six striking cases almost convinced us that the apex was sensitive, but of course we determined to make many more trials." As they had noticed that radicles grew much more quickly when subjected to considerable heat, and as they imagined that heat would increase their sensitiveness, they made five or six dozen trials on more than two dozen

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beans at a temperature of 69°–72° F. The result was moderately distinct deflection in only one radicle; in five other cases slight and doubtful deflection. "We were astonished at this result, and concluded that we had made some inexplicable mistake in the first six experiments. But before finally relinquishing the subject, we resolved to make one other trial, for it occurred to us that sensitiveness is easily affected by external conditions, and that radicles growing naturally in the earth in the early spring would not be subjected to a temperature nearly so high as 70° F." In the vast number of successful trials that they made they allowed the radicles to grow at a temperature of 55°–60° F.[1]

Had they stopped with the first trial, they would have hit the explanation which they finally adopted, and missed the effect of variations in temperature. Had they stopped with the second, the question would have hung in the balance between contradictory results. It was very feasible to reason that the results of the older experiment were due to some error of observation or manipulation. Nothing would have been known concerning the effect of temperature, and nothing concerning the original

[1] Power of Movement in Plants, pp. 141, 142.

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question. Their imagination had led them to introduce a new element into the second experiment; then reason prevented them from succumbing before the disturbance in the results, and led them to recognize it as a determining factor and treat it as such in their subsequent experiments.

The contradictory state in which things would have been left at the end of their second experiment is neatly illustrated by another case in connection with the same subject.[1] Ciesielski had shown, in his study of geotropic movements, that roots extending horizontally with their tips cut off did not grow downward. "He further states that, if the tips are cut off after the roots have been left extended horizontally for some little time, but before they have begun to bend downwards, they may be placed in any position and yet will bend as if still acted on by geotropism; and this shows that some influence had been already transmitted to the bending part from the tip before it was amputated." Sachs repeated these experiments, but denied the conclusions, because in his experiments the roots became distorted in all directions. The Darwins undertook to learn the cause of the contradiction in the results. After describ-

[1] Power of Movement in Plants, p. 523.

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ing unsuccessful efforts based on reasoning, they go on to say, "We next thought that, if care were not taken in cutting off the tips transversely, one side of the stump might be irritated more than the other, either at first, or subsequently during the regeneration of the tip, and that this might cause the radicle to bend to one side." They amputated some radicles obliquely and some transversely, and allowed them to grow perpendicularly. There was little or no distortion at first; but after two or three days, when the new tips began to form, the distortion of the obliquely amputated radicles became very conspicuous. The new tip was probably formed obliquely, causing the bending. Sachs probably "unintentionally amputated the radicles not strictly transversely"; and by not attending to this apparently insignificant condition he produced confusion and failed to make a discovery.

This case is interesting not only because it illustrates the difficulties that are met by the individual investigator, but because it is a typical example of a very large proportion of contradictions in results with which the literature of science is burdened. The contradiction in the results obtained by the two men was due, not to errors of observation, but to neglect of

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the various conditions under which the experiments were made. As I have elsewhere shown for another more involved case, the disputant observers were both right.[1] It was lack of exhaustion of the logical conditions of the problem that led to the contradiction. Total exclusion of error requires that every movement of the experimenter be fraught with intention. It is fairly safe to assume that, if two observers are competent and upright, their contradictory results, no matter on what subject, will prove essential to the final solution of the problem.

The publication of the "Power of Movement in Plants" was followed by several years of active investigation on and discussion of the "Darwinian curvature" of radicles. It has been shown that radicles, instead of being deflected by tin-foil on soft sand, will penetrate mercury and pierce tin-foil even when they strike it at a quite high angle. In explanation of the initial error of the Darwins it has been suggested that the radicles upon which they experimented were wilted. It has been further shown that in the experiments in which they attached small hard objects to the tips of the radicles to induce them to curve, the curva-

[1] Popular Science Monthly, January, 1894, pp. 373-376.

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ture was not due to sensitiveness, but to the action of the shellac by means of which the objects had been attached. Microscopic examination of radicles to which the materials used for attachment had been applied showed that the cells were affected. In short, the curvature of radicles ascribed to sensitiveness to touch has been shown to be due to pathological conditions brought into existence by the experiments themselves. The reasoning was correct enough, but the premises were false. Initial errors led to a false conclusion, but the experiments were all valuable as starting points for more searching investigations. The hypothesis of sensitiveness has been proved by Wiesner [1] and others to be untenable, but much more is known of the Darwinian curvature now than when the Darwins published their conclusions.

All Darwin's works on plants furnish examples of the practically complete development of the conditions of the problem. In the opening of the chapter on "Illegitimate Offspring of Heterostyled Plants," he said, "I give the results of my experiments in detail, partly because the observations are extremely

[1] Wiesner, J., Untersuchungen über die Wachsthumsbewegungen der Wurzeln (Darwinische und Geotropische Wurzel-Krümmung). Sitz. Akad. Wien, V. 89, I. pp. 223-302.

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troublesome and will not probably soon be repeated,— thus I was compelled to count under the microscope above twenty thousand seeds of *Lythrum salicaria.*"[1] The whole of his work on the same subject is on the same scale, vast numbers of observations being condensed into each of numerous tables.

It is elsewhere described how he had foreseen the importance of making comparative observations on the effects of cross- and self-fertilization in plants; and how he had been deterred by a bad analogy, and had finally had the subject thrust upon him while making experiments on *Linaria vulgaris* and the carnation with another end in view.[2] There are extant in all biological literature few equally fine examples of the clear comprehension of the conditions of a problem and of untiring attention to them.[3] He began by making a preliminary experiment with two plants, and, finding that, as in his previous accidental observations, the cross-fertilized seedling was in every respect superior to the self-fertilized, he proceeded to experiment on a very large scale.

[1] Different Forms of Flowers on Plants of the same Species, p. 189.

[2] Effects of Cross- and Self-Fertilization, p. 8.

[3] Ibid., pp. 10–27.

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He covered the plants with nets, and cross- and self-fertilized them artificially without castration, so as to make the cases parallel in all respects. The seeds were thoroughly ripened; the cross- and self-fertilized seeds were chosen in pairs that had germinated simultaneously, and were planted on opposite sides of the same pot. When one of a pair became sickly or injured, both were thrown away. All the seeds which remained after a number of pairs were thus planted were sown and left to grow up *crowded* on opposite sides of the same pot. The soil was carefully made uniform; the plants on the two sides of a pot were always watered at the same time and as equally as possible. The plants on the two sides of a pot were separated by a partition, but the pot was turned so that the two sides would be equally lighted. In this way the cross- and self-fertilized seedlings that competed with each other were subjected to as nearly similar conditions as human ingenuity could produce. Different sets of competing plants were subjected to different conditions, to determine whether the inequality between the cross- and self-fertilized seedlings would show itself only under favorable, or unfavorable, or under all circumstances.

In making the comparisons the eye alone

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was never trusted. Many plants were measured while young, when nearly full grown, and when matured. Equal numbers of the two kinds were cut down and weighed. Records were kept of the rate of germination, of the periods of flowering, and of productiveness both as to the number of capsules produced and as to the average number of contained seeds. Finally, the tables of measurements were submitted to Francis Galton in order to insure against error and to have them examined by the best statistical methods. Darwin intended at first to raise only one generation of crossed and self-fertilized seedlings of each kind, but in many cases he went as high as ten generations. Plants of different generations were exposed to different conditions in successive years. He started crossed seeds of *Ipomœa purpurea* (third generation) forty-eight hours later than the self-fertilized; seeds were sown outdoors late in the season, and only one stick given to each set to climb on; two lots were sown in a shady and weedy part of the garden; two other lots were sown in a bed of candytuft; seeds from the same plant were sown, the crossed in one corner and the self-fertilized in another corner of a tub in which a *Brugmannia* had been growing, and in which the soil was excessively

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poor; others were transferred from the hot-house to the coldest part of the greenhouse;—all to test the relative vigor of the crossed and self-fertilized seedlings.

He considered the possible sources of error, and showed that accidental cross-fertilization of plants intended for self-fertilization, and accidental self-fertilization of plants intended for cross-fertilization, would diminish rather than exaggerate his results. It having been said that an excess of pollen was injurious, two sets of sixty-four each of *Ipomœa purpurea* were tested to find whether the quantity of pollen applied to the stigma made any difference, and the statement was proved untrue. He published the details of his experiments, because they extended over eleven years and "are not likely soon to be repeated." When Darwin said in his conclusion that cross-fertilized plants are superior to self-fertilized plants and have a permanent advantage over them in the struggle for existence, and that nature abhors perpetual self-fertilization, there was no man to gainsay it; his ingenuity had spent itself in exhausting the conditions of the problem. He could have reached the same conclusion from his two accidental observations and his first direct experiment on the two plants.

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In this way he could have secured for himself the priceless gem of "*priority* of discovery" without the tedious years of work; he could then have produced what so many scientists in prominent positions produce on subjects fit to occupy one mind for years, – a few pages of general discussion and desultory reference to scattered and long-known facts.

The characteristic of exhaustiveness and its consequences is well illustrated in his "detail work" in the Monograph of Cirripedia. By the examination of an enormous number of specimens he showed how very variable are the species of the genus *Balanus*, and how, through imperfect examinations and want of caution, so many nominal species of fossil *Balani* have been described. Discussing dubious species, he said, "Bronn does not seem to have been aware of the absolute necessity of giving minute details in his descriptions of fossil cirripeds."[1] Probably in no department of the biological sciences has there been more superficial and worthless work done than in the description of species. This is doubtless due to the fact that a spurious fame could be acquired by the connection between the author's name and that of the species he described.

[1] Monograph of the Cirripedia, Vol. II. pp. 173, 184.

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Even good naturalists have frequently regarded a short description sufficient for ordinary purposes of identification as even preferable to a minute enumeration of details. Darwin not only gave an example of the permanent worth of the latter method of specific description in his "Monograph of the Cirripedia," but his book on the "Origin of Species" is one of the finest examples extant of the fact that a short statement of any subject to be valuable and forcible must be an abridgment of and be based upon a vast mass of details. In a correspondence with Hugh Strickland he expresses himself almost savagely in condemnation of the wretchedly poor work of species describers; he had unusually good reason to feel aggrieved because the nature of his work compelled him to use so much of the work of others. But he did far less for the improvement of specific description by personal example in the Monograph of Cirripedia, and by personal condemnation of the poor work of others, than he did indirectly by his general theories of descent and natural selection. The doctrine that species had their origin in varieties and in individual variations has changed the purpose of specific description. Identification and classification have been made processes subsidiary to some-

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thing higher. The establishment of a new standard of value for specific work has not only directed this work into new channels and the careful study of details, but has made many old descriptions valueless.

Intimately connected with the thoroughness with which scientific work is done is the length of time spent on it. One of the serious objections to waiting for better facilities, more evidence, etc., when the question of closing up an investigation arises, is the probability of loss of interest in the subject. Promptness in completing any line of work seems commendable on account of the economy of time, the greater certainty of recording results to date, and the importance of keeping the coast clear for new work. But work "completed" in a short time suffers from incompleteness, from whatever point of view it is regarded. Nothing can be so demonstrative as the relative permanence of work that has been done slowly and work that has been done with promptness and apparent vigor. The latter almost invariably takes a very subordinate place in the literature of the subject when once that subject is competely worked out.

In these days of competition, when every field of biology is ferreted for new subjects of

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investigation, and others are likely to secure priority of publication, there is every temptation to publish prematurely. Priority having been gained by a so called preliminary notice, a large proportion of the subjects are dropped by the original investigators, and let alone by others because they are "old." Time makes investigation easier. Where speed is felt to be necessary, a vast outlay of energy is frequently required to discover what with more time would almost come of itself. With the attention steadily fixed, time brings to bear multitudes of facts that would otherwise be lost. Gaps in the evidence, if filled at all, are too often filled with "necessary inferences" instead of facts.

There is perhaps no better case on record to illustrate the effect of time on the development of theory than the "Origin of Species." Darwin had already long reflected on the subject when he opened his first note-book for facts in 1837; and for more than twenty years thereafter he labored in analyzing and interpreting the facts of nature by the help of his theory in order to test the latter in all its relations. By carrying on simultaneously several investigations bearing on the general subject, he could let each of his studies drag through many years, and yet was able to accomplish

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much. Some of the most important explanations under his theories did not occur to him until years after he had begun their study. It will be pointed out later that after he once got possession of a working hypothesis his work was largely deductive; and it will be shown how extremely difficult it is to work out all the important consequences of such a hypothesis even in many years. After it is once done the task seems so easy that the wonder is that it was not done sooner. But the contemplation of the development of a great theory soon reveals the enormous difficulties in the way of one or many who seek to work out its consequences. Darwin did in each of his investigations what is usually done for a subject by a number of successive workers; each makes an important contribution to the subject, removes a serious objection to a theory, explains a section of the evidence, points out an important consequence, or modifies the statement of it to bring it more clearly in harmony with the greater knowledge on the subject. By successive approximations many men, working toward the same end, originate, build up, and improve a theory until it takes its place among permanently established truths. As far as it was possible for one man to do so Darwin did all

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this for his subjects before he gave them to the world. His work on the "Expression of the Emotions" began in 1838 and closed in 1872; "Insectivorous Plants," 1860–1876; "Vegetable Mould and Earthworms," 1837–1881. One of the most notable legacies that he left to the ambitious student is his example of great energy and great patience, his incarnation of the truth that time, as well as reason, is the handmaid of science.

Coupled with the habit of treating exhaustively the subjects with which he dealt, and the willingness to bide his time for publication until his views had reached their full maturity, was his extreme conscientiousness in giving full force to the objections against his general results. The habit of pursuing all the facts to their meaning made it possible for him to say that he had been able to consider in advance all the objections that were afterwards made to any of his views. The summing up of his experiments on *Adonis æstivalis*, in the "Effects of Cross- and Self-Fertilization," is an example of the great length to which he went in recording facts, especially if they were in any way opposed to his conclusions. He said, "The results of my experiments on this plant are hardly worth giving, as I remarked in my notes

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made at the time, 'seedlings from some unknown cause, all miserably unhealthy,' nor did they ever become healthy; yet I feel bound to give the present case, as it is opposed to the general results at which I have arrived."[1]

He did not hesitate to diminish the positive results of his experiments or the effect of his views by incorporating all exceptions, unless they were clearly due to some known extraneous cause. But his inability to leave any thing unexplained was so great that he rarely left exceptional facts without at least suggestions of possible explanations. He was extremely ingenious in guessing explanations for facts that could not be brought under the same general explanation as the other facts of their class. A good instance of this art of wriggling is his attempt to explain the sloping terraces of Coquimbo.[2]

[1] Effects of Cross- and Self-Fertilization, p. 128.

[2] Geological Observations, etc., pp. 256–258.

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V.

NEGATIVE EVIDENCE.

IT would naturally be expected, from Darwin's clear notions of evidence in general, and the necessity that he was under all his life of handling vast bodies of complicated evidence, that his work would furnish examples for the treatment of negative evidence. His estimate of its value is well shown by his treatment of the question whether *Primula veris*, *P. vulgaris*, and *P. elatior* are different forms of the same species.[1] After discussing the evidence in favor of this view he said, "Negative evidence is of little value; but the following facts may be worth giving." Then follows the recital of his efforts to determine whether the cowslip varies enough to justify the belief. He transplanted cowslips from the fields into a shrubbery, and then into highly-manured land; the next year they were protected from insects, artificially fertilized, and seed grown, which

[1] Different Forms of Flowers on Plants of the same Species, p. 62.

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was sown in a hot-bed. The young plants were set out, some in very rich soil, some in stiff, poor clay, some in old peat, and others in pots in the greenhouse, — seven hundred and sixty-five in all. Though they and their parents were subjected to all this diversity of treatment, "not one of them presented the least variation except in size." Negative evidence is indeed of little value, unless it can be shown that it covers the whole ground. In order to transform these experiments into proof it would be necessary to show that, if the three forms belong to one species, the cowslip should have varied under the conditions to which it was subjected. It is far more difficult to disprove a proposition by negative evidence than by proving the truth of its contradictory. Darwin accordingly demonstrated what several other botanists had *surmised:* that the oxlip is a hybrid between the cowslip and the primrose.

His efforts to determine whether *Orchis morio* secretes nectar also furnish a good illustration of his treatment of negative evidence.[1] A nectary implies nectar, but Sprengel had thoroughly searched many flowers of *O. maculata* and *morio,* and could not find a drop. Of his own efforts in this direction, Darwin said,

[1] Fertilization of Orchids, pp. 36–41.

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"I have looked to all our common British species and could find no trace of nectar; I examined, for instance, eleven flowers of *O. maculata*, taken from different plants growing in different districts, and taken from the most favorable position on the spike, and could not find under the microscope the smallest bead of nectar." Sprengel believed that these plants exhibit an organized system of deception, "for he well knew that the visits of insects were indispensable for their fertilization"; but Darwin could not believe in so gigantic an imposture. "Notwithstanding these several facts," he went on, "I still suspected that nectar must be secreted by our own orchids, and I determined to examine *O. morio* rigorously. As soon as the flowers were open I began to examine them for twenty-three consecutive days; I looked at them after hot sunshine, after rain, and at all hours; I kept the spikes in water and examined them at midnight and early the next morning." He irritated the nectaries with bristles and exposed them to irritating vapors. He examined flowers whose pollinia had been removed, and others which would probably have them soon removed. But the nectary was invariably dry. Only after he had made the negative evidence as complete as it could be made, by examining

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the nectaries of very many flowers from different places under all the possible circumstances in which nectar might be secreted, did he feel justified in saying, "We may therefore safely conclude that the nectaries of the above named orchids neither in this country nor in Germany ever contain nectar." Even then he restricted the negative conclusion to the two countries in which the exhaustive examinations had been made.

But he did not rest with this negative evidence. It was strong enough to convince him that there was no ordinary nectar, but the further evidence that he presents shows how quickly negative evidence falls into the background in the presence of even the most indirect positive evidence. He was thoroughly convinced that these orchids require the visits of insects for fertilization, that insects visit flowers for the attractions offered in the way of nectar, pollen, etc.; that nature could not deceive insects by a permanent imposture, and yet that in these orchids the ordinary attraction was absent. It was as if a crime had been committed, and he were asked to believe there was no criminal.

In examining the nectaries of the several species of orchids he was "surprised at the

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degree to which the inner and outer membranes forming the tube or spur were separated from each other, also at the delicate nature of the inner membrane, . . . and, lastly, at the quantity of fluid contained between the two membranes." He found the space between the membranes of other nectaries quite dry. He then examined other forms that do secrete nectar in the ordinary way, and found the membranes closely united, instead of separated by a space. "I was therefore led to conclude," he said, "that insects penetrate the lax membrane of the nectaries of the above named orchids and suck the copious fluid between the two membranes. This was a bold hypothesis, for at the time no case was known of insects penetrating with their proboscides even the laxest membrane." He afterwards learned that at the Cape of Good Hope moths and butterflies penetrate peaches and plums, and in Queensland, Australia, a moth penetrates the rind of the orange. These facts merely proved his anticipation less anomalous than it had seemed. The bees which he saw visiting *Orchis morio* kept their proboscides inserted in the nectaries for some time. He opened several nectaries, and found brown specks, due, as he believed, to punctures made some time before. Herman

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Müller has since corroborated Darwin's interpretation, saying, "My own observations have confirmed this view, as well as every detail of the rest of Darwin's account."[1] The negative evidence was, by its very completeness, a stumbling-block to Darwin's beliefs. As must sooner or later be done with all instances of negative evidence, he again set about replacing it by positive evidence, which removed the necessity of the belief which the negative evidence destroyed. Until that was done the negative evidence increased rather than diminished the mystery that needed solution.

[1] H. Müller, Fertilization of Flowers, pp. 535–538.

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VI.

CLASSIFICATION.

PERFORMED consciously or unconsciously, the act of classification is indispensable to and accompanies every scientific inference. A mind is orderly or slovenly, according as it does or does not habitually and accurately classify the facts with which it comes in contact. The success of an investigation, the worth of a conclusion, are in direct proportion to the fidelity to this principle and the exhaustiveness with which the process is carried out.

In nature, constant forces at work upon varying materials necessarily produce segregation; the like are brought together, and the unlike separated. The result is a literally "natural classification." This simple result is rarely realized. The forces at work are so numerous, and have acted so long, that, especially with reference to living things, Nature's serial classifications in space and time have been broken up and thrown into confusion to such an extent that they are seldom recognizable. The result

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is a superficial chaos of phenomena. The recognition of natural classifications was an excessively slow growth; they were finally worked out by the slow collection of material and successive attempts at a natural arrangement. In nature's arrangement of living things over the earth it has been very difficult to recognize law, and at first it was possible only where isolation has been long continued and the forces at work upon living things have been few and steady in their action. Even then the recognition has required extensive travel and a powerful inclination to classify and recognize the relations of distant facts to each other.

It was the recognition of several such arrangements or classifications in Nature that first led Darwin to reflect on the Origin of Species. What immortalized his observations is not the simple fact that they were made, but that by their cumulative presentation they led Darwin to seek an adequate cause for these natural arrangements.

After pointing out, in the narrative of his voyage, the striking relation between the fossil and the living animals of South America, he said: "This wonderful relationship in the same continent between the dead and the living will, I do not doubt, hereafter throw more light on

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the appearance of organic beings on our earth, and their disappearance from it, than any other class of facts."[1] In passing southward over the continent of South America, he recognized another of Nature's classifications: he noticed the frequent recurrence of the fact that a species occupying a given region was replaced to the southward by a closely related species, and this serial arrangement impressed itself strongly upon him. His visit to the Galapagos Islands gave him an almost perfect example of simplicity in the working of Nature's forces. The conditions for one of Nature's classifications were perfect. When Lawson, the Vice Governor, had declared to him that the tortoises from the different islands differed from one another, Darwin did not see the significance of the fact. He mixed up his collections from the various islands, and did not dream that there lay before him one of the most remarkable facts that Nature ever revealed to a naturalist. By some happy accident he compared the many specimens of mocking thrushes shot on Charles Island with those from Albemarle Island, and was astonished to find that they belonged to different species. It was not the fact that there were two species of mocking thrush liv-

[1] Naturalist's Voyage around the World, p. 173.

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ing together on the two islands, but that they lived apart, each on its own island, and that they were closely instead of distantly related, — that several islands were stocked, each with its own species, or perhaps variety, of the same kind of animal, — that struck him with wonder. These facts haunted him, and drove him to look for others like them. Upon his arrival at home his insect collection proved the same law. He had fortunately kept the plants of the different islands separate, and Hooker, at Darwin's request to see whether the law held good for them, found it to be so.[1] Darwin had unwittingly carried one of Nature's beautiful classifications home to England with him. Keen insight into the relation of facts to one another had enabled him to recognize three striking examples of Nature's arrangements.

The habit of grouping facts to extract the truth from them was indispensable to Darwin's work, for he constantly dealt with large bodies of facts that were manageable in no other way. It will be seen how difficult it is even for a powerful observer to see facts for which he is not looking, even though they lie under his feet. An act of classification, to be worth much, must usually be an effort to answer

[1] Naturalist's Voyage around the World, pp. 393-398.

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a direct question. Science has derived very little or no benefit from the miscellaneous collecting and grouping of facts without any previous notion of what they are likely to reveal. An investigation is usually made for the purpose of answering a definite question, or of verifying an anticipation. With some such end in view, with some principle by which the classification is guided, the result usually reveals not only what was looked for, but frequently still more fundamental characteristics; for it is impossible to throw facts into any order which reveals one truth without dragging others into the light with it. The character of Darwin's work required constant recourse to lists and tables; he appreciated fully both their value and their treachery, and his great ability to recognize all the points brought out, no matter whether he was looking for them or whether they bore directly on the subject which he happened to be investigating or not, made them enormously useful to him.

Darwin's original purpose in measuring the heights of the gravel-capped plains of Patagonia was to ascertain the heights at which recent fossil shells occurred. These measurements gave him all he sought, — a notion of the amount of elevation in the recent period. On compar-

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ing his measurements with those of the Beagle survey, he was struck with their uniformity. He tabulated all the measurements representing the summit edges of the plains; and the tabulation proved to him that the elevation of the land had gone on at a remarkably equable rate over a north and south distance of at least five hundred miles.[1] There is comparatively little danger of throwing away effort in a well directed classification. The danger lies in not comprehending the vast significance of the process both in actual investigation and in the presentation of results, and in the lack of persistent determination to exhaust its resources. Darwin himself sometimes owed it to happy accident that he did not overlook this powerful instrument.

Guided by deduction to the probable relation of the distribution of volcanoes to that of coral islands, and to the distribution of fringing and barrier reefs and atolls in relation to each other, he spent months in mapping them from the descriptions of voyagers, surveying vessels, etc. From the classification of a vast chaotic mass of facts scattered throughout geographic and geological literature, he extracted some of the most important conclusions of his whole work

[1] Geological Observations, etc., p. 211.

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on coral islands.[1] Time has shown that the conclusions reached from this mapping of facts is too general. The conclusions that volcanoes are invariably absent from the areas which have recently subsided or are still subsiding, and are commonly present in areas that are rising or have recently risen, that fringing reefs lie in the areas of elevation and atolls in the areas of subsidence, may not be accepted without important reservations. But whether time shall ultimately substantiate or correct Darwin's conclusions, or shall even destroy some of them, his classifications will always remain essential to the study of coral islands.

[1] Structure and Distribution of Coral Islands, p. 189.

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VII.

ANALOGY.

ANALOGICAL reasoning plays a very important part in all scientific work; and Darwin frequently availed himself of its help in making discoveries and establishing conclusions. He used every logical device to establish and extend his theories, and there is no lack of material from which to choose examples. But analogy, when used on a large scale, proves so treacherous, that it is useful for the most part only in giving clues to discoveries. There are but few examples of analogical reasoning on a large scale in Darwin's works. The most important, perhaps, is his work on Insectivorous Plants.

It has already been told how, while resting at Hartfield after years of labor on the Origin of Species, he was struck by the number of insects caught by the leaves of the common sundew. It soon became evident to him that "*Drosera* was excellently adapted for the special purpose of catching insects, so that the subject

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seemed well worthy of investigation."[1] As soon as he began to work on *Drosera*, and was led to believe that the leaves absorbed nutritious matter from the insects, he began to reason by analogy from the well understood digestive capacity of animals. One needs but to imagine an attempt to do the work without any knowledge of animal digestion to understand at once its impossibility under such conditions. By connecting his observations with the well known animal processes he proceeded on a course of rapid discovery that must otherwise have remained entirely closed to him. At almost every step he drew suggestions from and checked his results by reference to animal digestion. He made preliminary "crucial" experiments by immersing some leaves of *Drosera* in nitrogenous and others in non-nitrogenous fluids of the same density to determine positively whether the former affected the leaves differently from the latter.[2] The discovery that *Drosera* detects "with almost unerring certainty the presence of nitrogen" in various fluids "led me to inquire," he said, "whether *Drosera* possessed the power of dissolving solid animal matter; the experiments proving that the leaves are capable of true

[1] Insectivorous Plants, p. 2. [2] Ibid., p. 76.

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digestion, and that the glands absorb the digested matter."[1] "These are, perhaps, the most interesting of all my observations on *Drosera*, as no such power was before distinctly known to exist in the vegetable kingdom." Having by analogy established the power of true digestion in plants, analogy led him to seek in plants the elements that do the work of digestion in animals. Bringing together what was known of plants, he pointed out that the juices of many plants contain an acid, and so one element of a digestive fluid was at hand; and that all plants possess the power of dissolving albuminous or proteid substances, protoplasm, chlorophyll, etc., and that "this must be effected by a solvent, probably consisting of a ferment together with an acid."[2] After writing the last quoted sentence he learned that a ferment which converted albuminous substances into true peptones had been extracted from the seeds of the vetch.

Sachs mentioned the discovery of the ferment, recorded the fact that peptones had themselves been actually found in the seeds of the lupine, and added "as we come to know the proteinaceous reserve materials of plants better, and if we follow their behavior in the

[1] Insectivorous Plants, p. 268. [2] Ibid., p. 362.

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animal body also, it can scarcely be doubtful that, in spite of incomplete knowledge, the assumption is nevertheless warranted that peptonizing ferments are perhaps universally distributed in plants."[1] "Attention was first drawn to the occurrence of peptonizing ferments in the vegetable kingdom by the remarkable phenomena observed in the so called insectivorous plants." By analogical reasoning a whole new field of study was opened; a new view of the powers of plants was gained, and a much closer analogy between plant and animal functions was established. But if recent studies are taken into account, the question may be raised whether this stupendous analogical structure has not been undermined. Tischutkin contends that the "digestion" of insectivorous plants is not accomplished in the same way as in animals, but is due to bacteria; that the pepsin of the leaves is not a secretion of the plant, but a by-product of the activity of the bacteria.[2] He proves that bacteria capable of dissolving egg albumen are always present in the secretion of the leaves; that they come principally from the air, that the plant only furnishes a medium for them to live in, that

[1] Sachs, Physiology of Plants, p. 344.

[2] Berichte der Deutschen Botanischen Gesellschaft, 1889.

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disintegration of albuminous substances begins only after enough micro-organisms are developed to do the work, and that the plant simply assimilates what these lower organisms have set free. The relation between the insectivorous plants and the bacteria is one of genuine symbiosis.

If the whole of Tischutkin's contention is true, the great body of facts brought out by Darwin must still be placed to the credit of analogical reasoning. The facts concerning plant and animal digestion would still remain parallel, both in the succession of the phenomena and in the results. It would be another illustration of the vast importance of analogy in scientific method, and of the fact that every analogy, the strongest as well as the faintest, will sooner or later fail.

In another instance, analogical reasoning from animals to plants actually deterred him from discovering the truth to which other logical processes might have led him. He states the case so clearly himself that it will almost suffice to quote him.[1] "The adaptation of flowers for cross-fertilization is a subject which has interested me for the last thirty-seven years. . . . From my own observations

[1] Effects of Cross- and Self-Fertilization, pp. 6–8.

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on plants, guided, to a certain extent, by the experience of breeders of animals, I became convinced many years ago that it is a general law of nature that flowers are adapted to be crossed, at least occasionally, by pollen from a distinct plant." It was a direct deduction from his theory of natural selection that, since they are adapted for cross-fertilization, cross-fertilization must be advantageous to them. Hence it was perfectly natural that he should like to verify it. "It often occurred to me," he said, "that it would be advisable to try whether seedlings from cross-fertilized flowers were in any way superior to those from self-fertilized flowers. But as no instance was known with animals of any evil appearing in a single generation from the closest possible interbreeding, that is, between brothers and sisters, I thought that the same rule would hold good with plants, and that it would be necessary, at the sacrifice of too much time, to self-fertilize and intercross plants during several successive generations, in order to arrive at any results. I ought to have reflected that such elaborate provisions favoring cross-fertilization as we see in innumerable plants would not have been acquired for the sake of a distant and slight advantage, or of avoiding a

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distant and slight evil. Moreover, the fertilization of a flower by its own pollen corresponds to a closer form of interbreeding than is possible with ordinary bisexual animals; so that an earlier result might have been expected."

He had carried the deduction far enough to warrant an effort to verify it, but was deterred by analogical reasoning from pursuing the matter further. Had he clung to his general theory and the special facts to be explained under it, he would, as he said himself, have reached an early result. The analogy, if it had served a good purpose, ought to have led him to reason that since continuous interbreeding is harmful among animals, although there are no special adaptations to prevent it, or to encourage the opposite, then, surely, the harmful effects of close breeding and the benefits of cross-fertilization ought to be very marked in plants, with their striking adaptation for cross-fertilization. The analogy had clearly led him astray; and he was finally brought back to the subject by a different route.

"I was at last led to make the experiments recorded in the present volume from the following circumstance. For the sake of determining certain points with respect to inheritance, and without any thought of the effects of close

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interbreeding, I raised, close together, two large beds of self-fertilized and crossed seedlings from the same plant of *Linaria vulgaris* (common Toad Flax). To my surprise, the crossed plants, when fully grown, were plainly taller and more vigorous than the self-fertilized ones.

"Bees incessantly visit the flowers of this *Linaria*, and carry pollen from one to the other; and if the insects are excluded the flowers produce extremely few seeds, so that the wild plants from which my seedlings were raised must have been intercrossed during all previous generations. It seems therefore quite incredible that the difference between the two beds of seedlings could have been due to a single act of self-fertilization; and I attributed the result to the self-fertilized seeds not having been well ripened, improbable as it was that all should have been in this state, or to some other accidental and inexplicable cause." During the next season he raised two beds of carnations (*Dianthus caryophyllus*) in the same way and for the same purpose; the preceding generations in this case also must have been continuously cross-fertilized; again "the self-fertilized seedlings were plainly inferior in height and vigor to the crossed."

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"My attention," he said, "was now thoroughly aroused, for I could hardly doubt that the difference between the two beds was due to the one being the offspring of crossed and the other of self-fertilized flowers." After the effects of cross- and self-fertilization had been thus thrust upon him, he proceeded to make the exhaustive examination that ran through many years, and finally filled a volume. He foresaw the meaning of the adaptations for cross-fertilization and the character of the results, but was deterred by a false analogy from making the observations to which a careful study of the facts by themselves would have infallibly led him; and was finally driven to the subject again by the empirical observation of the facts that he had anticipated by reasoning. It may seem strange that the very consequences which theory led him to expect had to be twice forced upon the attention of one who was so quick to seize Nature's suggestions, before he could be brought to investigate them. But the strangeness of such an intellectual phenomenon is all due to the afterthought. Even in the sciences that are most rigidly deductive it is a common thing for the investigator to stumble indirectly upon results which he might have foreseen or often did more or less perfectly foresee.

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In a more complex case, analogy led to a conclusion which, although it could not be verified, possesses great importance in relation to one of the principal difficulties in the way of the general theory of natural selection. In the course of the investigation on "Different Forms of Flowers on Plants of the same Species," he noticed the striking parallelism between the phenomena of hybridism and those of the heterostyled plants which he was studying.[1] When once the parallelism was established, the remarkable and puzzling facts of hybridism doubtless furnished a solid analogical basis from which to foresee and scrutinize the results of crossing the different forms of heterostyled plants.

Difficulty in uniting two forms and sterility of their offspring had been almost universally regarded as a test of specific distinctness. Darwin showed clearly that this belief, although very generally true, is by no means universally so; and his work on heterostyled plants showed that all the phenomena of hybridism were displayed among forms that certainly belonged to the same species. He triumphantly overthrew the doctrine that

[1] Different Forms of Flowers on Plants of the same Species, pp. 242, 243.

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mutual sterility is a mark of specific distinctness. But Huxley, in his essay on the "Coming of Age of the 'Origin of Species,'" said: "In my earliest criticisms of the 'Origin' I ventured to point out that its logical foundation was insecure so long as experiments in selective breeding had not produced varieties which were more or less infertile; and that insecurity remains up to the present time."[1] Such was the serious nature of the facts of hybridism which needed explanation. And it was the close parallelism between hybridism and heterostylism that led Darwin to seek in the latter an explanation of the difficulties presented by the former. From the study of the illegitimate offspring of heterostyled plants he drew the conclusion that the sterility is due, not to structural differences, but to functional differences between the sexual elements; and that it is not due directly to natural selection, but is an incidental result accompanying the adaptation of the sexual elements of the different forms of plants of the same species to fertilize each other. By inverting the analogy he transferred the conclusion to the facts of hybridism. He said that it was this consideration, that the sterility of species when first

[1] Life and Letters, Vol. I. p. 551.

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crossed, and of their hybrid offspring, is due to functional differences of the sexual elements, and not to structural differences between the species, that led him to make the many experiments on the illegitimate offspring of heterostyled plants, and that made the results worthy of publication. The great strength of the analogy in his mind was doubtless due to the fact that the parallelism was so very close as to force the conclusion that the two sets of results, one within a species, the other between species, are due to the same *cause*.

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VIII.

INDUCTION.

WHEN Darwin had once grasped the idea of the descent of species, and natural selection as the cause determining modification, it was inevitable that he should look upon all classes of biological facts as consequences of these. Accordingly, nearly all the investigations which he carried on were pursued as deductions from his general principles. But although the larger outlines and a large part of the details of his work were deductive, he was frequently obliged to pass by induction from facts to the subordinate principles which he established.

One of the earliest of the many instances in which he felt compelled to re-interpret whole groups of facts was that relating to human expression. When, in 1838, he read Sir Charles Bell's great work on the "Anatomy of Expression," the view of the latter that man had been created with certain muscles especially adapted for the expression of his feelings struck him

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as unsatisfactory. As a deduction from his general theory, he believed that the habit of expressing our feelings by certain movements had been somehow gradually acquired. This view required that the whole subject of expression should be studied under a new aspect, and each expression be given a rational explanation.[1] This led him to undertake his work on the "Expression of the Emotions in Man and the Lower Animals." Deduction pointed out that expression must fall under the general explanation; but it was impossible to foresee the principles which governed the development of the various expressions. From 1838 to 1872 he toiled away at the mass of complex facts, and slowly overcame the difficulty of bringing the different expressions together under one or a few points of view. At last he was able to educe three principles of expression, which seemed to him to account for most of the expressions and gestures involuntarily used by man and animals under the influence of various emotions and sensations. These principles he arrived at, as he himself says, only at the close of his observations.[2] They

[1] Expression of the Emotions in Man and the Lower Animals, p. 19.

[2] Ibid., p. 27.

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are: (1) the principle of serviceable associated habits; (2) the principle of antithesis; (3) the principle of actions due to the constitution of the nervous system, independently from the first of the will, and independently to a certain extent of habit. It is to be remarked of these three principles that they are inductions, and that they are vague on two accounts: they are in part so general that it might prove difficult to bring them to a crucial test with the hope of proving or disproving them; and the only test to which they have been put is that of explaining the very facts from which they were drawn. They have not been used to make further discoveries; they represent well the type of inductions based on many carefully studied facts, but unsupported by a subsequent deductive research.

A simpler case of induction is his inference concerning the age at which characters appear which are inherited by one or both sexes. "It is in itself probable," he said, "that any character appearing at an early age would tend to be inherited equally by both sexes, for the sexes do not differ much in constitution, before the power of reproduction is gained"; and went on to point out that characters appearing late in one sex would tend to be restricted to

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that sex. "I was led to infer that a relation of this kind exists from the fact that whenever and in whatever manner the adult male has come to differ from the adult female, he differs in the same manner from the young of both sexes." [1]

The principle that characters appearing at an early age are inherited by both sexes, and characters appearing late in one sex are restricted to that sex, is an induction from certain striking differences between the adult males and females, and between the adult male and the young of both sexes in many species. By itself, without verification or other deductive bracing, it would have been an interesting generalization. But Darwin sought to strengthen it both by observing whether it held true in particular cases, and by deducing it from more general laws. In the first of the two quotations given above, he pointed out the probability that the truth of the principle depends on the known changes that take place in the constitution of the sexes on approaching maturity. It occurred to him to put the principle to a crucial test, and to rely on the result. Thereupon followed the investigation on the deer family, described elsewhere in more detail.

[1] Descent of Man, etc., p. 276.

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Inductions are easily made; the test of a good investigator, however, is not the number of inductions he makes, but his subsequent treatment of them. In his study of the amount of material brought to the surface of the ground by earthworms, Darwin noticed that the surfaces of old worm-castings were often studded with coarse particles; and was thence led to infer that a good deal of the finest part of worm-castings was washed away by the rain. Such an induction is so nearly self-evident that it would seem superfluous to verify it. Darwin thought otherwise; he mixed fine precipitated chalk with the castings, or gently stuck it on to them; the rain washed it away and proved him correct. By this little induction, and a verification almost childish in its simplicity and apparent insignificance, he was able to show that the amount of material brought to the surface by earthworms is much greater than at first appears.[1] It is probably safe to say that the majority even of investigators would have regarded the induction sufficient unto itself, and would not have hesitated to use it without verification as evidence in proof of their views. We here get a glimpse of how sharply Darwin caught the significance of the minutest indica-

[1] The Formation of Vegetable Mould, etc., p. 272.

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tions, and of the patience with which he made experiments to prove things which to others would seem so simple and self-evident as to need no proof. Like his great contemporary, Faraday, he "could trust a fact, and always cross-examined an assertion." It was his conscientious verification of even his smallest inductions that gave the scientific world its great confidence in his work.

Darwin's greatest induction has yet to be considered, and will be discussed at some length; because, as well as being his greatest induction, it is his most notable speculative failure, and will give an opportunity to study the characteristics of false and true hypotheses. The problem of inheritance, the transmission of qualities from parent to offspring, had weighed upon him during all the years of his work on the theories of descent and natural selection. Almost at the very start we find him making experiments on beds of self- and cross-fertilized plants to determine questions of inheritance. These experiments possess their greatest interest, not from having furnished any important data for the solution of the problem of inheritance, but from having finally impressed him with the importance of cross-fertilization in the plant kingdom. They show how early he

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appreciated the connection between inheritance and his general theories, and tried to get an experimental basis for inference.

Darwin, as he himself confessed, had to make a theory on every subject, and the intimate relation between inheritance and his other theories led him irresistibly to form a theory on inheritance. Like Newton, he established the best of theories, and, like him, "was also capable of proposing one of the worst." He finally published the hypothesis under the title "Pangenesis," in Volume II. of "Variation of Animals and Plants under Domestication."[1] The reason he gave for forming it was to bring the vast number of perplexing facts of inheritance together under a single intelligible point of view. To do this he assumed the existence of minute bodies called gemmules, which are cast off by all the living cells of the animal or plant body at all stages of its existence, and which multiply by division, have the power of remaining dormant through an indefinite number of generations, possess certain remarkable affinities, etc. They were supposed to collect in the reproductive elements, and determine the character of the offspring. Darwin endeavored to make the assumption

[1] Pages 349–399.

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reasonable by pointing out analogies between the gemmules and the reproductive elements, between their affinities and those of pollen, etc.; and rebutted the objection of excessive minuteness by a comparison with molecules. Doubtless the gemmules were a development of the idea of reproductive elements, or blood-corpuscles, or both; and their peculiar origin, powers, and hypothetical history were determined by the various facts that had to be explained. It was a clear case of pure induction, untainted by any direct or indirect evidence of the existence of the gemmules, or any glimpse of the process by which characters are transmitted. Darwin knew how speculative the hypothesis was, and justified it because it brought all the facts of inheritance together under one point of view.

In his discussion of it he first stated the facts to be brought together, and then the hypothesis with a working explanation of it, and finally tried to show, by reasoning deductively from the hypothesis to the facts in which it had originated, that it explained them.

Usually during the effort to reach a cause by the road of induction the cause itself is caught sight of, either directly or indirectly, and it is then possible to formulate at once at least a

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provisional hypothesis. In the case of Pangenesis this was not true. Darwin gave the following as the principal reasons for believing in natural selection: "(1) It is a true or recognized cause; (2) from the analogy of change under domestication by man's selection, and (3) chiefly from this view connecting under an intelligible point of view a host of facts." To this may be added a fourth, which he mentioned to Huxley in connection with the third, as being the reasons why younger scientists would choose his theories rather than the doctrine of creation; namely, that it would enable them to search out new lines of investigation.

Let Pangenesis be tried by these four tests. In the first place, it was not even claimed that there was any proof, either direct or indirect, of the existence of the gemmules. As a cause which had been actually observed, they had no existence. Secondly, while analogy is often a strong collateral argument, and was so in the case of natural selection, it was a treacherous support in the case of Pangenesis. Perhaps as many analogies were violated, as was pointed out by Delpino and others, by the conception of the gemmules as it could muster to its support; and one of the first essentials of an argument from analogy is that the points of

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similarity shall be more numerous than the points of difference. The third and chief reason which Darwin gave for belief in natural selection, that it connected under an intelligible point of view a host of facts, is identical with the reason which he gave for forming and so tenaciously clinging to the hypothesis of Pangenesis. And this condition the latter fulfilled. It was bound to do so by the very terms of its origin. A hypothesis was not likely to leave Darwin's hands until it did harmonize with the facts from which it had taken its rise.

The fourth reason for belief in a theory, namely, that it leads deductively to new investigations, and through them to new facts, brings up the hypothesis of Pangenesis against a wall. A cause may be recognized as a working force; its claim to having produced known effects may be supported by analogy; and a vast body of phenomena that must be effects of some cause may be brought together into harmony under it. But it must also be possible to work out other consequences of the theory.

Francis Galton made a determined effort to test the hypothesis. He felt the pressing importance of doing so, for, as he said, "Its postulates are hypothetical and large, so that

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few naturalists seem willing to grant them."[1] He reasoned that, if the gemmules or bearers of inheritable qualities existed in such enormous numbers in the body, they must be borne from place to place and to the reproductive organs by the blood. If this were true, then by injecting blood from one variety of rabbit into the blood-system of rabbits of another variety, the gemmules introduced with the foreign blood would pass with those proper to the animal itself into the reproductive elements, and would modify the character of the offspring. He foresaw the practical importance of such a result. Slight dashes of blood could be introduced by breeders to modify a variety; for example, greyhounds could have a little of the bull-dog instilled into them. At the end of his investigation he said, "I have now made experiments of transfusion and cross-circulation on a large scale in rabbits, and have arrived at definite results, negativing, in my opinion, beyond all doubt, the truth of the doctrine of Pangenesis." Thus ended the only effort ever made to test the hypothesis deductively by reasoning out its consequences and trying to establish them by experiment.

[1] Proceedings of the Royal Society, 1871, Vol XIX. p. 394 *et seq.*

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It would seem as if Galton's experiments had proved the hypothesis false. Darwin, however, met the criticism, and slipped away from the results by admitting that he would have expected gemmules in the blood; but showed that their presence there was no necessary part of his hypothesis, because the latter applied to the lower animals and to plants, which do not have blood.[1] Darwin's modest defence of his hypothesis swept away not only Galton's experiments, but the possibility of proving or disproving it. Lionel Beale remarked, sarcastically, that it might still be possible to test it by cutting out a mass of an animal's flesh, and grafting in its place a piece from an animal of another variety.[2] Darwin's rejoinder to Galton was sound from the former's point of view. But the hypothesis was incapable of definite proof or disproof. There was no set of facts left that could be appealed to as a test. A good induction will not only be in harmony with and bring under one point of view a host of facts, but is likely to be supported by one or more of the following lines of proof: (1) independent direct evidence of the existence of the cause involved in the induction; (2) strong and

[1] Nature, April 27, 1871, Vol. III. p. 502.

[2] Nature, May 11, 1871, Vol. IV. p. 25.

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independent analogies; (3) the possibility of deducing consequences from the hypothesis, and verifying them by observation or experiment; (4) the possibility of deducing the cause or principle as an effect of another still more general.

Huxley advised Darwin not to publish his doctrine of Pangenesis; but he nevertheless did publish it, and gave as his reason the pressing importance of co-ordinating the inexplicable facts of inheritance. Other hypotheses have followed Darwin's with as little success. Darwin did not formulate his hypothesis to support his other theories, but its character was at least in part determined by the latter. It is interesting to note that the latest hypothesis of inheritance, the most aggressive that has yet arisen, has been developed as a special support for the belief that natural selection acting upon congenital variations is the sole cause in the production of species, and that acquired characters are not inherited. The principal evidence on which the hypothesis relies for support, apart from the refutation of the direct evidence adduced to support the belief in the inheritance of acquired characters, are the karyokinetic processes in cell division, and the early stage in development at which, in some animals, the

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reproductive elements become distinct from the parts which develop into the other organs of the body. In this respect Weissmann's hypothesis has an additional logical support. It has provoked earnest discussion, and naturalists have taken sides on the subject. Although the germ plasm is itself an assumption, and the hypothesis owes its existence to the known facts of karyokinesis, it has led to further investigations in some directions with fruitful results. Darwin's hypothesis opened the question under the new light that he had shed upon nature, and the more recent hypotheses, like his own, owe their existence to the "impelling force" of his general theories of descent and natural selection.

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IX.

DEDUCTION. — EXPLANATION OF KNOWN FACTS. — IMPORTANCE OF THEORY TO GOOD OBSERVATION.

DARWIN fell upon the true cause of the modification of species so early, that the greater number of his special investigations took on a deductive cast.[1] His reflections upon the known facts and principles of biology took the form of efforts to explain them as deductions from his theory; and many of his new discoveries were foreseen as consequences of it. Hence the inductive process does not play so important a part in his work as does the inverse process of deduction.

It will not be necessary to dwell long upon his success in giving the proper theoretical explanations to already known facts and empirical laws. It had required centuries of painstaking research and numberless efforts at classification before anything like a natural classification was

[1] See the Chapter on the Logical History of the Principle of Natural Selection, *post*, p. 212.

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reached. When, at the beginning of this century, it was finally approached, and the natural affinities of plants and animals were brought out by it, the doctrine of descent was inevitable; and it came. When Darwin had once become impressed with its truth, and had found the cause of modification, it was first of all necessary to show that the great bodies of known facts harmonized with his doctrines. The facts of distribution, palæontology, embryology, rudimentary organs, etc., were all reduced. Each set of facts presented its own difficulties.

Up to the present century it was regarded as an axiom in taxonomy that the structures of most importance to the animals possessing them must be of most importance for the purposes of classification. It is worth while to note that this was accepted as self-evident, as being beyond the necessity of proof. Systematists were approaching the "natural arrangement," and De Candolle discovered empirically the rule that there is usually an *inverse* ratio between the taxonomic and the functional value of a structure; but he could suggest no reason for the paradox. Darwin's theory furnished the philosophical explanation.[1] The

[1] Origin of Species, pp. 362–373. Romanes, Darwin and After Darwin, pp. 34–37.

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organs of the highest functional value are under the constant and pressing necessity of changing with changes in the environment; while those of least functional value remain undisturbed, and pass, little or not at all modified, from generation to generation. The same explanation holds for the rule concerning the importance of "aggregates of unimportant characters" in determining the affinities of animals and plants.

In some parts of the natural system, there are what are called "chains of affinities." A group, instead of being broken up into well separated sub-groups, consists of a chain in which the adjacent parts are closely related, but the more distant parts have comparatively few points in common. It is impossible to break up the group without violating the affinities of adjacent parts, and it is difficult to define it in such a way as to include the extremes. The Crustacea furnish an example. What was a special difficulty under the old views of classification is explained under the doctrine of descent.[1]

One of the most important principles that had been established empirically was the tree-like arrangement of species and higher groups

[1] Origin of Species, p. 368.

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in the natural classification. The recognition of the principle came only after centuries of efforts at classification; and after it was discovered, no reason could be given for it. Although so helpful and striking, it remained a profound enigma, for there was nothing in the nature of the things classified that required this peculiarly complex arrangement rather than one of several conceivable simpler ones. Darwin's explanation of this principle under his theory illustrates not only the explanation of empirical laws, but the difficulty of doing what, after it is done, seems very simple.

It was not until after he had been at work upon the principle of natural selection for many years, that the true explanation of this law under his principle occurred to him. He said, "I suppose I must be a very slow thinker, for you would be surprised at the number of years it took me to see clearly what some of the problems were which had to be solved; such as the necessity of the principle of divergence of character, the extinction of intermediate varieties, on a continuous area, with graduated conditions," etc.[1] After describing his earlier sketches of his theory he said, "At that time I overlooked one problem of great im-

[1] Life and Letters, Vol. I. pp. 68, 524.

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portance, . . . the tendency in organic beings descended from the same stock to diverge in character as they become modified." That they have thus diverged was proved by their arrangement into groups within groups. "I can remember," he went on, "the very spot in the road, whilst in my carriage, when to my joy the solution occurred to me; and this was long after I had come to Down."[1] The solution was this, — "that the modified offspring of all dominant and increasing forms tend to become adapted to many and highly diversified places in the economy of nature."

It seems very simple now to understand that the tree-like arrangement of species must be regarded as a direct consequence of the principle of natural selection. It might even seem easy to infer the tree-like arrangement as a deduction from the principle, and to discover it, if it had not already been discovered empirically. It is not because Darwin was so slow a thinker that it took him so long to discover the relation of cause and effect between natural selection and the tree-like arrangement of species; but it was because he was a strong and persistent thinker that he discovered it at all. The thinking out of such connections is always a slow

[1] Life and Letters, Vol. I. pp. 68–70.

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process. The final mental act may come like a flash, as it did in Darwin's case, and be followed by a very rapid and fruitful explanation of details; but such triumphs do not come to the mind that will not serve the apprenticeship.[1]

It has been often declared that the work of biologists since 1860 has consisted in explaining known facts as deductions from Darwin's theories, and further investigating the consequences of those theories. The more extensively a theory has been successfully applied, the more easy it is to do what still remains to be done. There is profound philosophy in the saying, "To him that hath shall be given." It was a comparatively easy matter to apply Darwin's theories to all sorts of facts and lines of investigation after he had so thoroughly tested and illustrated them; but even then the scientific world was very long in working out some of their striking consequences. It is not to be wondered at that Darwin was slow to understand many things, or that he overlooked others entirely. It is rather to be wondered at that he accomplished so much single-handed. One

[1] My friend, Prof. G. C. Price, has called my attention to the fact that Lincoln was marvellously like Darwin in many respects. The former was noted for his efforts to reach fundamental principles in thinking, and was also noted for the "slowness" of his mental action.

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of the hardest and most important lessons for the majority of investigators to learn is that the chances are all against their exhausting their subjects, or even putting them into such shape that the work will not all have to be done over again, unless their work is done slowly and continued persistently through long periods of time.

Darwin's great logical power was fortified by another rare quality of mind, — unusual acuteness in observing all collateral facts that came out in his observations or experiments, whether they seemed to bear directly upon the subject of investigation or not. He was thus put in possession of many facts that afterwards proved valuable in ways that he could not foresee. But with all his ability in these directions he experienced difficulty in grasping the full significance of facts.

In discussing Cleistogene flowers, near the end of his work on the "Effects of Cross- and Self-Fertilization," [1] after giving the reasons for the belief he is about to express, he said, "I must believe that plants now bearing small and inconspicuous flowers profit by their still remaining open, so as to be occasionally intercrossed by insects. It has been one of the

[1] Page 387.

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greatest oversights of my work that I did not experimentize on such flowers, owing to the difficulty of fertilizing them and to my not having seen the importance of the subject."

Although it is clear that the possession of a theory is no guaranty that all its consequences will be foreseen, or, if foreseen, observed, or even that, if they are both deductively foreseen and empirically observed, they will be brought into connection with the theory; nevertheless, the importance of theory for accurate observation cannot be overestimated. However cautious Darwin was about committing himself unreservedly to a hypothesis, he never really dispensed with one if he could find one. Though he subdued his tendency to speculation in the interest of observation, he did not dispense with at least provisional hypotheses, even in accumulating his facts. He felt the want when he could not find one, and made it his first task to establish some degree of probability in favor of one.

One of his early experiences is a good illustration of how even trained observers could not, without the help of a theory, observe phenomena on which they actually walked, and which obstructed their progress.[1] He has said, "I

[1] Life and Letters, Vol. I. pp. 48, 49.

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had a striking instance of how easy it is to overlook phenomena, however conspicuous, before they have been observed by any one. We spent many hours at Cwm Idwal, examining the rocks with extreme care, as Sedgwick was anxious to find fossils in them, but neither of us saw a trace of the wonderful glacial phenomena all around us; we did not notice the plainly scored rocks, the perched boulders, the lateral and terminal moraines, yet these phenomena are so conspicuous that, as I declared in a paper published many years afterwards in the Philosophical Magazine (1842), a house burnt down by fire did not tell its story more plainly than did this valley. If it had been filled with a glacier, the phenomena would have been less distinct than they now are."

It may seem strange that two men, one already a famous geologist and the other soon to become one, should have overlooked such evidence, which has since become so interesting, so widely known, and so well understood. The secret of their failure is that they were not looking for it. It is usually the things that men look for that they see; and to look for things as yet unseen requires a theory as a headlight. Even if they had noticed the material of the moraines, the perched boulders, and

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the parallel scratches, they would certainly not have been deeply impressed by them, because they become impressive only when their relation to one another is understood, and this could only be when the glacial theory had been imported from a glacier country.

The importance of the discovery of the theory of natural selection to the work of Darwin's life will be dwelt upon later. A sigh of relief is embodied in the declaration, "Here, then, I had at last got a theory by which to work."[1] Facts cannot be seen without some notion of the relation they will bear to each other when they are found. The stupendous importance of theory for observation is illustrated by the effect of Darwin's theories on biological investigation in all its phases. Huxley put it thus: "The 'Origin' provided us with the working hypothesis we sought."[2] The whole biological world was waiting for it; and when it came it carried the biological sciences into the deductive stage, and opened an era of investigation unprecedented in the rapidity with which discovery advanced, and in the accuracy of the results reached.

There are scattered throughout Darwin's

[1] Life and Letters, Vol. I. p. 68.
[2] Ibid., p. 551.

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works numerous illustrations of the importance of theory in the investigation even of matters of detail. Writing of the trimorphic *Lythrum salicaria*, he said, "The existence of the three forms was first observed by Vaucher, and subsequently by Wirtgen; but these botanists, not being guided by any theory or even suspicion of their functional differences, did not perceive some of the most curious points of difference in their structure."[1] MM. Boitard and Corbié, in their study of pigeons, had seen and recorded many facts which they could not use, simply from lack of a theory. They had stated that when they crossed certain breeds of pigeons, birds colored like the *Columba livia*, or the common dove-cot, were almost invariably produced.[2] Darwin gave significance to these facts and many others by the theory of descent.

In spite of his unusual power of seeing facts apparently unconnected with the subject under investigation, and his persistent habit of recording results, whatever they might be, Darwin himself, sometimes "not foreseeing the result, did not keep a memorandum of all the facts," which would afterwards have proved useful.

[1] Different Forms of Flowers on Plants of the same Species, p. 138.

[2] Variation of Animals and Plants under Domestication, Vol. II. p. 14.

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No one was more thoroughly convinced of the necessity of clear-cut theory for accurate observation; and he frequently expressed himself to that effect. His son says of him, "He often said that no one could be a good observer unless he was an active theorizer."[1] He said himself, "I am a firm believer that without speculation there is no good and original observation."[2] "It is an old and firm conviction of mine that the naturalists who accumulate facts and make many partial generalizations are the *real* benefactors of science. Those who merely accumulate facts I cannot very much respect."[3]

[1] Life and Letters, Vol. I. p. 126.
[2] Ibid., p. 465.
[3] Ibid., Vol. II. p. 21.

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X.

DEDUCTION. — ANTICIPATION.

A VERY few negative instances have been given to show the importance of theory for accurate observation. They illustrate how the absence of theory led to the oversight or neglect of facts which later, under the sway of theory, have become important. Darwin's works are full of instances in which he was led by his theory to anticipate the facts of nature. It was inevitable that, having so early discovered the theories which covered the whole territory in which he worked, he should be guided by them in the search for facts, and that his work should thenceforth be deductive in its character. Examples of this characteristic method range from the great deductions which led to nearly all his important special investigations, and which illustrate the sweeping consequences of his general theories, to the little deductive details which show how swift and accurate his prevision became even in the matter of minute consequences of these theories.

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The minor instances will be given first, and will be followed by the more general ones. Then will follow deductions which he made, but which are still unverified, or have been verified by others; and lastly will be given some of the instances in which he went clearly wrong in his deductions.

The instance about to be given may well be placed first, for the purpose of raising a mooted question in logic. Mill took the position that typically the process of inference consists in reasoning directly from one particular case to another; whereas the older and more generally accepted view is that inference must pass by induction from particulars to a general law, and then by deduction from the general law to other particulars. Darwin had found silicified wood in certain tufaceous formations in Patagonia and on the island of Chiloe on the west coast of South America. He afterwards crossed the Andean Cordillera in an east and west direction, and again found tufaceous formations. In his description of the geological section of the Uspallata range he said: "Many of these tufaceous beds resemble, with the exception of being more indurated, the upper beds of the great Patagonian Tertiary formation, especially those variously colored layers high up the river

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Santa Cruz, and in a remarkable degree the tufaceous formations at the northern end of Chiloe. I was so much struck with this resemblance, that I particularly looked out for silicified wood, and found it under the following extraordinary circumstances." [1]

We are not told whether he gained his first knowledge of the connection between silicified wood and tufaceous formations from his geological researches in South America; nor if he did, whether he had come to the general conclusion that silicified wood is likely to be found in tufaceous formations, and therefore it ought to be found in the beds of the Uspallata range, or whether he simply reasoned that because silicified wood is found in the tufaceous beds of Chiloe, therefore it will be found in the very similar tufaceous beds of the Uspallata range. It is known that the two are found in connection in different parts of the world, and there is a causal connection between them. The peculiar conditions under which tufaceous beds are deposited, and the subsequent processes of mineral solution, etc., are such that, if wood was present at the time of the deposition of such beds, it would probably be silicified. Whether Darwin knew of this causal connec-

[1] Geological Observations, etc., p. 526.

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tion or only knew empirically that the two are often found in connection, if he reasoned from a belief that the two are likely to be found together, the Uspallata case is one of deduction. It would be interesting to have a complete record of the mental operations in this case; but from Darwin's mental habit of leaping quickly to hypotheses, even where the connection between facts appeared altogether empirical, there cannot be much doubt about the deductive character of the process, even if it be held that the process of inference is typically from one particular case to another.

In his studies of coral islands he laboriously gathered information concerning the distribution of atolls, barrier reefs, and fringing reefs, and indicated each kind by a different color on the map, distinguishing fringing reefs with red. He had reached the conclusion that atolls and barrier reefs lie in areas of subsidence, and fringing reefs in areas of elevation. So far as he could learn from records, Banks Islands and some others apparently had no fringing reefs, although they lie in what he had designated the zone of elevation. He pointed out the fact that most of the information concerning coral reefs and islands had been collected and recorded in the interests of navigation, and

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from this point of view narrow fringing reefs would be insignificant, and likely to be overlooked. He said, "I do not doubt that several of these islands, now left uncolored [on his map], ought to be red." Bonney, in his revised edition of Darwin's "Coral Reefs," says, on the authority of Captain Wharton, that Banks Islands are fringed in parts.[1]

The data from which he had compiled the map of the distribution of coral reefs and islands had been recorded for a purpose entirely different from that for which he wished to use them; hence much information that was important for him was left out. The imperfect data verified his principle of distribution; and he was able to infer deductively that there were small fringing reefs in existence where they had not been observed or recorded.

In pursuit of the same subject he studied carefully the charts of the Great Chagos Bank, which he had not seen himself. He saw from a study of the chart that, as he said, "On the eastern side of the atoll some of the banks are linear and parallel, like islets in a great river, and they pointed directly towards a great breach on the opposite side of the atoll. I inferred from this that strong currents sometimes set

[1] Structure and Distribution of Coral Reefs, p. 220.

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directly across this great bank; and I hear from Captain Moresby that this is the case."[1] The causal relation between currents of water and linear parallel islets was known to him; from this law and the particular arrangement of the banks he deduced the existence of currents, and verified the anticipation.

Some of these instances are so simple as almost to require apology for their insertion; but their very simplicity makes them typical of the most common of the logical processes. The variety of consequences to be deduced from principles or laws is so great, that inductions by which those laws or principles are reached are few compared with the deductions by which their consequences are developed.

Darwin's studies of the fertilization of flowers are full of examples of deduction and verification; and some of these are very curious. The principle of advantage from cross-fertilization lay at the root of these deductions; and it was by incessant application of it that he was able to interpret the most complex arrangements in orchidaceous flowers. He studied the structure of *Listera ovata*, and experimented on the action of its parts, until he felt sure that he understood the manner in which insects enter the

[1] Structure and Distribution of Coral Reefs, p. 150.

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flower and free the pollinia. After watching flowers for hours to see Nature at work, he was rewarded with a verification of his interpretation.[1] In the description of another case he said, "From the large size of the flower, more especially of the viscid disk, and from its wonderful power of adhesion, I formerly inferred that the flowers were visited by large insects, and this is now known to be the case."[2] He was well acquainted with the general interaction of the parts and their relation to visiting insects. From this and the size, etc. of the parts, he was able to deduce the size and strength of the insect. He had been convinced by theoretical considerations that the pollen of *Hedychium* is removed by the wings of hovering butterflies, and wrote to India to have the butterflies observed in action. Two years later Fritz Müller observed the process itself.

Probably no other of Darwin's works illustrates so well the variety of results which may be deduced from a general principle by ingenuity and skill in interpretation as does that on the "Fertilization of Orchids." When once he had laid down the proposition that the flowers of many plants are adapted for cross-

[1] Fertilization of Orchids, pp. 119, 120.

[2] Ibid., p. 190.

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fertilization, he set himself the task of interpreting the complex arrangement of parts in orchidaceous flowers in relation to the visiting insects in accordance with that proposition.

One of the most interesting and successful applications of this principle is the following. He has described various excrescences, warts, ribs, ridges, etc. on the labellum, or large odd petal of different orchids, the flowers of which do not secrete nectar. He was haunted by the question of the meaning of these absurdly insignificant and irregular things, — their relation to the other parts and to the visiting insects. He said, "From the position relatively to the viscid disk [by means of which the pollen is removed by visiting insects] which these excrescences occupy, and from the absence of any free nectar, it formerly seemed to me highly probable that they afforded food, and thus attracted either Hymenoptera or flower-feeding Coleoptera. . . . Nevertheless it was a bold speculation that insects were attracted to the flowers of various orchids in order to gnaw the excrescences or other parts of the labella; and few things have given me more satisfaction than the full confirmation of this view by Dr. Crüger, who has repeatedly witnessed in the West Indies humble-bees of the genus *Euglossa*

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gnawing the labellum of *Catasetum*, *Coryanthus*, *Gongora*, and *Stanhopea*."[1]

When his attention was first attracted to the cowslip (*Primula veris*), and he noticed the different forms of flowers on different plants, some with longer pistils, rougher stigmas, and smaller pollen grains, and some with shorter pistils, longer stamens, and larger pollen grains, he inferred that the species was tending to become dicœcious. Later he became convinced that the differences between the forms were for the purpose of securing cross-fertilization, and proved this to be the case. In his discussion of the positions of the flower organs in the long-styled and short-styled forms he said: "The anthers in the one form stand nearly, but not exactly, on a level with the stigma of the other. . . . It follows from the position of the organs that if the proboscis of a dead humble-bee, or a thick bristle, or a rough needle be pushed down the corolla, first of one form and then of the other, as an insect would do in visiting the two forms growing mingled together, pollen from the long-stamened form adheres around the base of the object, and is left with certainty on the stigma of the long-styled form; whilst pollen from the short stamens of the

[1] Fertilization of Orchids, pp. 269, 270.

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long-styled form adheres a little way above the extremity of the object, and some is generally left on the stigma of the other form. In accordance with this observation I found that the two kinds of pollen, which could easily be recognized under the microscope, adhered in this manner to the proboscides of the two species of humble-bees and of the moth which were caught visiting the flowers."[1]

From these apparently insignificant deductions and verifications he passed on to show that the short-styled form was far more likely to be self-fertilized than the long-styled form. For when he inserted a bristle or other object into the corolla of the short-styled form, he had to pass it between the anthers seated round the mouth of the corolla, and some of the pollen was almost invariably carried down and left on the stigma. His inferences concerning adaptation for cross-fertilization in these forms were completely verified, so far as it could be done from a study of the relative positions of the parts of the flowers and the action of insects upon them. The proof was strong enough to support the principle. But for Darwin it was merely made the starting point for further verification from the physiological side. "The

[1] Different Forms of Flowers, etc., pp. 18–24.

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several foregoing facts led me to try the effects of the two kinds of pollen on the stigmas of the two forms." This "trial" consisted of a long series of experiments to establish the effects of legitimate and illegitimate unions of the two forms. The reward of this effort to establish the principle by physiological evidence is summed up in his own words: "From the facts now given the superiority of a legitimate over an illegitimate union admits of not the least doubt; and we have here a case to which no parallel exists in the vegetable, or indeed in the animal kingdom."[1]

It had long been known that if pollen from a distinct species is placed on the stigma of a plant, and afterwards (sometimes many hours afterwards) pollen from its own species is placed on the same stigma, the latter obliterates the effects of the former, and the plant will be fertilized by the pollen of its own species. He had shown that the two forms of flowers of the cowslip were beautifully adapted in structure for cross-fertilization, and that it was essential to the vigor of the species that there be cross-fertilization between the two forms, and that cross-fertilization between flowers of the same form, or fertilization of a

[1] Different Forms of Flowers, etc., p. 28.

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flower by its own pollen, should be prevented. But he had also shown that there is a mechanical liability to self-fertilization in the short-styled form by the pollen being carried down from the stamens to the pistil. These facts led him to the belief that the pollen of the other form is prepotent over the pollen of the same form as that to which the stigma belongs, when they are placed on it together. He said, "There can hardly be a doubt that with heterostyled dimorphic plants, pollen from the other form will obliterate the effects of the pollen from the same form, even when this has been placed on the stigma a considerable time before. To test this belief I placed on several stigmas of a long-styled cowslip plenty of pollen from the same plant, and twenty hours later pollen from a short-styled dark red polyanthus" (variety of cowslip). Of the thirty seedlings, all bore reddish flowers, showing that they were the result of the cross.[1]

It is not surprising that, after he had verified his general inference concerning prepotency in heterostyled dimorphic plants by the experiments with the highly specialized flowers of the cowslip, he should take another step and test the inference by a species in which there

[1] Different Forms of Flowers, etc, p. 31.

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was much less difference between the two forms. After describing the two forms of *Linum grandiflorum*, which differ only in the *length* of their *pistils*, he said that he had in his garden two fine long-styled plants, separated by a considerable distance and some evergreens from plants of the other form. He marked twelve flowers on these two long-styled plants, and put some short-styled pollen on them; but they, and the rest of the vast number of flowers on these two plants, had their stigmas already covered with their own pollen, and it was late in the season (September 15). The pollen of the two forms could not be distinguished under the microscope. "Altogether," said Darwin, "it seemed almost childish to expect any result. Nevertheless, from my experiments on Primula I had faith, and did not hesitate to make the trial, but certainly did not anticipate the full result that was obtained. The germens of the twelve flowers swelled, and bore six good capsules (the seed of which germinated) and two bad ones; only four falling off. All the other flowers were absolutely barren; not even their germens swelled." [1] Not content with this striking result, he hunted down this absolute sterility of the long-styled plants

[1] Different Forms of Flowers, etc., p. 83.

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with their own form pollen, which his experiments of 1861 had brought out, by carrying through a long series of experiments which, as he said, were so curious that he gave them in detail. His conclusion from the experiments is that although the pollen of the two forms is identical under the microscope, "taking fertility as a criterion of distinctness, it is no exaggeration to say that the pollen has been brought to a degree of differentiation, with respect to its action on the stigma of the same form, corresponding with that existing between the pollen and stigma of species belonging to distinct genera." [1]

An excellent illustration of the relation between induction and deduction in the scientific method is furnished by Darwin's study of the expression of grief. During several years, he said, no expression seemed to him so utterly perplexing as this one. Why should the inner ends of the eyebrows be raised when a person feels the emotion of grief? Or, in other words, what is the cause of the obliquity of the eyebrows under suffering? "Why should grief or anxiety cause the central fasciæ alone of the frontal muscle, together with those round the eyes, to contract?" [2] Here was a little induc-

[1] Different Forms of Flowers, etc., pp. 87–90.
[2] Expression of the Emotions, etc., pp. 188–192.

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tive problem: the effects given, to find the cause, without any hint as to its nature. Several years would seem long enough for a mind like Darwin's.

He had seen Duchenne's photograph of a young man contracting the grief-muscles while looking up at a strongly illuminated surface, but had entirely forgotten the picture. The problem remained unsolved. One day while on horseback, with the sun behind him, he met a young girl whose eyebrows as she looked up at him became extremely oblique, with the proper furrows on her forehead. He had been unable to reach the cause by contemplation of the effects. Now he had observed a cause producing effects identical with those which puzzled him. There was now a basis for experiment. He went home and caused three of his children, without their knowing why, to look up at the top of a tall pine against a bright sky. "With all three," he said, "the orbicular, corrugator, and pyramidal muscles were energetically contracted, through reflex action" to protect the eyes against the light. But they wanted to see, and a "curious struggle arose between these muscles, that tended to lower the brows and close the lids and all or only the central fasciæ of the frontal muscle,

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causing obliquity of the brows with puckering and swelling of their inner ends, so that the exact expression, in every detail, of grief or anxiety was assumed "

"When children scream they contract the orbicular, corrugator, and pyramidal muscles, primarily for the sake of compressing their eyes, and thus protecting them from being gorged with blood." By observation on the little girl and his own children he found that the peculiar expression characteristic of grief was the combined result of the reflex contraction of the muscles around the eye to protect it by closing it, and the voluntary contraction of other muscles to keep it open. In children the unrestrained expression of suffering called into action only one set of these muscles. These were the factors in his possession for the solution of the problem. He knew that the elevation of the inner ends of the eyebrows was due to the effort to keep the eyes open. "I therefore expected to find with children," he said, "that when they endeavored either to prevent a crying-fit from coming on, or to stop crying, they would check the contraction of the above-named muscles, in the same manner as when looking upwards at a bright light; and consequently that the central fasciæ of the frontal

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muscle would often be brought into play." He himself observed children with reference to this point, and had others, including physicians, do so. He soon found that the "grief-muscles" were very frequently brought into very distinct action, and has given a number of cases. The crying-muscles act in children, and have acted for countless generations. The pyramidal muscles are least under the control of the will, and can be counteracted only by the voluntary contraction of the central fasciæ of the frontal muscle; and that is the expression of grief.

This instance of the solution of an inductive problem is by its apparent smallness a striking example of the difficulties of scientific investigation, and of the necessity of appealing to all the logical processes. After the explanation is once made, it would seem an easy matter to analyze the complex effect called the expression of grief, and discover its causes in the involuntary contraction of one set, and the voluntary contraction of another set of muscles; but the apparent simplicity is all due to the explanation itself. It will be remarked, in the discussion of the logical history of the principle of natural selection, that the causes of any set of facts are rarely discovered by a direct study

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of the effects. Sooner or later a cause is observed to produce the effect of which an explanation is desired; then by an induction the cause is applied to the whole class of effects, and the induction is established by subsequent deduction and verification.

One of the most interesting examples of reasoning recorded in Darwin's works is that concerning the colors of caterpillars. Although Darwin put the problem and Wallace solved it, it is inserted here because it illustrates the devices used to secure explanations of facts. Darwin had undertaken to explain the beauty of butterflies by the principle of sexual selection, but knew that it was foolish to think that the beauty of the mature animal was thus explained unless the equally beautiful colors of many caterpillars, in whose case sexual selection *certainly could not act*, were explained in some special way.[1] Here was another set of facts that could not be reduced under his theory; and again appears the almost insuperable difficulty of passing directly from facts to their causes. He whom many regarded as a master in the art of "wriggling" was unable to devise an explanation, and appealed to Wallace,

[1] Descent of Man, etc., Vol. I. pp. 202–204; Life and Letters, Vol. II. p. 276.

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who, he said, had an innate genius for solving difficulties. Wallace, by a pretty paradox, referred this anomalous set of facts to the well known principle of protective coloring. He said most caterpillars require protection, as may be inferred from some kinds having spines or hairs, others being colored like leaves and twigs. But in known cases in which the coloring served to protect the larva it did so by concealing the animal from its enemies. Gaudy colors would expose it to the sharp sight of every foe; how, then, could they serve to protect it? By reasoning from these considerations "Mr. Wallace thought it probable that conspicuously colored caterpillars were protected by having a nauseous taste." But as they are tender, a peck from a bird's beak would be fatal. "Distastefulness alone," said Wallace, "would be insufficient to protect a caterpillar unless some outward sign indicated to its would-be destroyer that its prey was a disgusting morsel." Darwin presented the reasoning to the Entomological Society, and it was supported by statements of the members, and afterwards verified by Jenner Weir's experiments with his birds. The smooth green and twig-like larvæ were devoured by the birds; and the spinous and brightly colored

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ones were rejected with signs of distastefulness.

The correlation of brilliant colors and distastefulness was thus anticipated as a consequence of natural selection, the principle of protective coloring and the belief that animals so protected by obscure colors are attractive to their enemies. Darwin immediately seized on this result as an opening for further investigations, and said: "This view will, it is probable, be hereafter extended to many animals which are colored in a conspicuous manner." The study of animal coloration under Darwin's principle of natural selection and the subordinate principles of mimicry, etc., has been carried to such a length, or rather the effort to explain coloration under these and similar principles has been carried to such a length, that one prominent zoologist has felt justified in characterizing the speculations of recent years on the coloration of animals as a mild form of scientific lunacy. There has been an enormous amount of wild deduction and half-digested observation; but what is most needed is more light on the physiological causes at work within the animal and producing and determining the distribution of colors.

It has been mentioned in another place that

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Darwin had concluded that characters inherited by both sexes appear early in life, and that characters restricted to one sex appear late. He said, "I was led to infer that a relation of this kind exists, from the fact that whenever and in whatever manner the adult male has come to differ from the adult female, he differs in the same manner from the young of both sexes." This induction he proceeded to strengthen by deduction at both ends. He deduced the law from a still more general one, as follows: "It is in itself probable that any character appearing at an early age would tend to be inherited equally by both sexes, for the sexes do not differ much in constitution, before the power of reproduction is gained"; and in the same way characters appearing late in one sex would tend to be restricted to that sex.[1]

From the other side, he sought to test the law by an examination of its consequences; and chose the deer family as a crucial instance, upon which he felt he could rely.[2] In some species of this family the males alone have horns, and in one species both sexes have them. If the law were true, horns should appear late in the species in which they are restricted to the

[1] Descent of Man, etc., Vol. I. p. 276.

[2] Ibid., p. 278 *et seq.*

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males, and early in the species of which both sexes have horns. He found that in seven species with horns only on the males the horns appear late, — at nine months, or even later. In the reindeer, which alone has horns on both sexes, the horns appear in both sexes at the age of four or five weeks. His investigations took a somewhat wider range, and he gave other illustrations and some exceptions to the law among horned animals.

One of the most striking of his minor efforts was his explanation of the origin of the remarkable color patterns called "ocelli" on the tail of the peacock.[1] He had adopted the theory of sexual selection for the explanation of the beauty of birds, etc., which did not seem to him to admit of explanation under natural selection. There are other naturalists who do not believe that sexual selection plays the part in nature which Darwin ascribed to it. But whatever view is taken of it, it furnished him with the indispensable working hypothesis by means of which to explain known facts and search for others that ought to follow as consequences of the hypothesis. It was characteristic of Darwin to select for investigation and explanation extreme instances that would put his beliefs to

[1] Descent of Man, etc., Vol II. pp. 132–145.

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the most rigorous test. The ocelli on the tail of the peacock furnished such an instance. The problem was how to explain the origin of these ocelli from the ordinary feather markings of the group to which the peacock belongs.

First, his belief that the color patterns of many birds are due to sexual selection led him to explain the absence of ocelli from the tail-feathers of the two species of peacock by the fact that these feathers are covered up and concealed by the long tail-coverts. The theory required that the patterns due to sexual selection should be exposed to the gaze of the females. "In this respect," he said, "they differ remarkably from the tail-feathers of Polyplectron, which in most of the species are ornamented with larger ocelli than those on the tail-coverts." According to theory, the ocelli had disappeared from the tail-feathers and been remarkably developed on the tail-coverts because the latter by enormous development had covered the former. In Polyplectron, in which the tail-coverts are not so enormously developed, the ocelli were larger on the tail-feathers than on the coverts. By the theory, however, the markings of the Polyplectron are more generalized, and some indications of gradations between these two extremes ought

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to exist. "Hence," he said, "I was led to carefully examine the tail-feathers of the several species of Polyplectron in order to discover whether the ocelli in any of them showed any tendency to disappear, and, to my great satisfaction, I was successful." His whole study of these ocelli is remarkable for the ingenuity with which he worked out the consequences of his theory and verified them; and explained the intricate pattern as a modification and specialization of more general feather-marking.

Darwin's theory of descent led him to regard species as only more strongly marked varieties. "From looking at species as only strongly marked and well defined varieties, I was led to anticipate that the species of the larger genera in each country would oftener present varieties than the species of the smaller genera." He tested this deduction by an extensive tabulation of plants and Coleoptera; "and it has invariably proved to be the case that a larger proportion of the species on the side of the larger genera presented varieties than on the side of the smaller genera." [1]

When once he had got hold of natural selection as the cause that had brought about the adaptations in nature, it was an essential part of

[1] Origin of Species, p. 44.

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the doctrine that only variations favorable to the species could be preserved. It was an inevitable inference from the nature of the cause of adaptations that there could be no such thing in nature as an adaptation for the exclusive benefit of any other species than the one possessing it. Darwin was so confident of this deduction, although it could never be rigidly verified by observation, that he boldly staked the fate of his whole theory on the truth of the inference. "If it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced through natural selection."[1] He could never know from direct observation that there were no such cases in existence, but he was driven to the assertion of their non-existence by the nature of the cause of adaptations. Doubtless this deduction gave the severest blow that was ever dealt to the belief in general benevolence in Nature, and it has proved the final blow. It was the apparently awful consequences of this inevitable deduction that roused the bitter opposition of the religious forces of the world. It seemed to establish forever the doctrine that

[1] Origin of Species, p. 162.

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advantage to self is the only invariable motive of all the striving in the universe. The moral consequences of the theory seemed to outrage all the noble ideals that had ever been cherished in the world.

Since the time he made the declaration, there have been up for discussion numerous cases of adaptation which seemed of no value, or even hurtful, to the species possessing them. These difficulties have caused a vast amount of strenuous explanatory wriggling under the name symbiosis; but there has as yet been no case found which can be positively regarded as an example of what Darwin said could not exist. He had long studied adaptations carefully; but the nature of the cause, after he had discovered it, helped him to understand more clearly the nature of adaptations.

Good testimony for the necessity of a theory of some kind to enable him to work effectively is what Darwin wrote to Asa Gray about cleistogamic flowers, which are self-fertilized and do not open at all. He said, "The *temporary* theory which I have formed on this class of dimorphism, just to guide experiment, is that the *perfect* flowers can only be perfectly fertilized by insects, and are in this case abundantly crossed; but that the flowers are not

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always, especially in early spring, visited enough by insects, and therefore the little imperfect self-fertilizing flowers are developed to insure a sufficiency of seed for present generations." This *temporary* theory is now generally accepted as the true one.[1]

[1] Life and Letters, Vol. II. p. 482.

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XI.

DEDUCTION. — GENERAL INSTANCES.

IT has been said that a large proportion of Darwin's special investigations were started as deductions from the general theories of descent and natural selection for the purpose of corroborating or testing them, and otherwise working out their consequences. But before turning to these it will be well to notice a piece of deductive work not connected with those theories; for it is important, both on account of his using it himself as an illustration of his method and on account of the more recent views that have arisen in opposition to it.

The hypothesis concerning the formation of coral reefs took its rise as a special application of the general rule that depositions are made over sinking areas to the coral limestones of the west coast of South America.

The main outlines of his work on coral islands were deductive, and so were many of the special investigations of details. He spoke

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of the theory of the formation of coral islands as the only one of his many hypotheses which he had not to modify afterwards, and asserted that therefore his study of coral islands was more deductive in spirit than any other of his investigations.[1] After the theory had held sway for forty years in the geological world as a complete explanation of coral reefs, it was called in question by John Murray, who sought to replace it by another which seemed to him more reasonable. Murray has found some adherents; the war of the two theories has been waged more or less hotly ever since, and both views are still in the field. Darwin's theory is not superseded,[2] nor is it likely to be; but it will nevertheless be somewhat modified to adjust it to the greater knowledge of the present. Indeed, if Darwin had lived to issue another edition of his book, he would have taken the new facts into account, as he did in his latest correspondence. Here, again, he had considered in advance many of the objections that have since been raised against his theory. But the chief interest of this case is derived

[1] Life and Letters, Vol. I. pp. 58, 83.

[2] Structure and Distribution of Coral Reefs, 3d edition, Appendix by Prof. T. G. Bonney; Ibid., Bettany's edition, Critical Introduction by Prof. John W. Judd.

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from the facts that he regarded it as the most deductive in spirit of all his work, and as the only hypothesis which he was not obliged to modify, and that scientists have since attacked it more severely perhaps than any of his other theories.

Darwin's approach to the subject of the expression of the emotions has already been described under Induction. He said, "When I read Sir C. Bell's great work [On the Anatomy of Expression], his view, that man had been created with certain muscles specially adapted for the expression of his feelings, struck me as unsatisfactory."[1] This was because he had become convinced of the truth of evolution, which required him to believe that the habit of expressing our feelings by certain movements had been somehow gradually acquired. This view required that the whole subject of expression should be studied under a new aspect; "and each expression," he said, "demanded a rational explanation. This belief led me to attempt the present work." In this case the general theory led him only to the conviction that there must be *some* rational explanation for each emotional expression; and left him to find out inductively the particular

[1] Expression of the Emotions, p. 19.

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nature of the explanations. It will be recalled that he reached three principles of expression; but only when he had completed his observations.

Darwin's botanical work was almost entirely done under the influence of the theory of evolution. His special investigations in this field were based on corollaries from the general theory. Writing to Mr. Murray, his publisher, concerning his book on the "Fertilization of Orchids," he said, "It will perhaps serve to illustrate how natural history may be worked under the belief of the modification of species."[1] Equipped with the belief that all adaptations are useful to the species possessing them, and that innumerable flowers, among them the orchids, are adapted for cross-fertilization, he studied orchids and the effects of cross- and self-fertilization in the same spirit in which he had studied coral islands.

One of the most interesting of the many investigations that arose out of his general theories was that on climbing plants. As has been said, he selected as subjects for special research some of the principal difficulties that presented themselves for explanation. He did this because, if the theories were true, they

[1] Life and Letters, Vol. II., Letter to Murray.

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ought to lead to the right explanations, and because he always regarded it as better to work out a single typical case or a few typical cases thoroughly, in proof of his theories, than to offer miscellaneous suggestions on many cases.

Climbing plants offered a case of special importance. When once the principles of descent and natural selection are adopted as working hypotheses, it is a comparatively easy matter to explain how a group of closely related animals or plants come to possess one or more striking characteristics in common. In such a case they are all supposed to have inherited the characters in question from a common ancestor. For example, all the woodpeckers, with their peculiar feet and tail and tongue, the last with its remarkable apparatus of hyoid-bone and muscle, — or the various oaks with their acorns and cupules, their flowers and leaves, — are believed to have descended from a common stock. The individuals of these groups have a great many characters in common. But climbing plants are found throughout the plant kingdom. Some families contain several or many closely related climbers, and these offer no special difficulty so far as their mutual possession of the power to climb is concerned.

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But other large families, normally composed of non-climbers, contain one or a few climbing species, as, for example, the hop in the nettle family.

The power to climb is so striking a character, and is so plainly useful to the plant possessing it, that Darwin's theories would be taxed with failure if they did not explain its origin. But climbing plants are found throughout the plant kingdom; and they are not descended from a common climbing ancestor, for they possess nothing in common except this one power to climb. "Plants," he said, "become climbers in order, it may be presumed, to reach the light, and to expose a large surface of leaves to its action and to that of the free air. This is effected by the climbers with wonderfully little expenditure of organized matter in comparison with trees, which have to support a load of heavy branches by a massive trunk. Hence, no doubt, it arises that there are in all quarters of the world so many climbing plants belonging to so many different orders." The very great advantage offered to the climber has acted as a powerful premium for the development of the capacity

[1] Journal of the Linnean Society, 1865; Botany, Vol. IX. pp. 107, 108.

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wherever variation offered the materials out of which natural selection could produce it.

The first thing to be established in proof of the derivation of climbing plants from non-climbers was the existence of gradations in the power of climbing, and of intermediate stages between the different methods of climbing, — by twining of the stem, by leaf-stalks, and by tendrils; just as he connected the ocelli of the peacock's tail by a series of gradations with the more ordinary feather-markings of related birds. But another unknown element was the source of the variations upon which natural selection could work to produce climbers. In his arguments to prove his theories of descent and natural selection Darwin showed that variations *do* occur, and that when they occur natural selection will inevitably preserve the favorable and destroy the unfavorable. But he could do little or nothing in the direction of pointing out the cause of variations. He has been incessantly twitted about this by his opponents, especially because of the false notion that he ascribed to chance all variations whose causes were not known. Ignorance of the sources of variation is no obstacle whatever to belief in Darwin's theories; but this is true only when the species having a certain

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character in common are proved by many other characters to be descended from a common stock. In the case of climbing plants the same kind of variation must have occurred independently in all parts of the plant kingdom, or there must have been a common source or tendency which served as a starting point for the development of the power to climb.

In his work on climbing plants, first published as a paper in the Linnean Journal,[1] he worked out in a masterly way the gradations in the power to climb, and between the different methods of climbing, showing that all are modifications of the method of climbing by twining of the stem. Near the end of his work he said, "We have seen how diversified are the movements of climbing plants. . . . They belong to many and widely different orders. . . . When we reflect on this wide serial distribution of plants having this power, and when we know that in some of the largest well defined orders, such as the Compositæ, Rubiaceæ, Scrophulariaceæ, Liliaceæ, etc., two or three genera alone out of a host of genera in each, have this power, the conclusion is forced on our minds that the capacity of acquiring the revolving

[1] Journal of the Linnean Society, 1865, Botany, Vol. IX., "On the Movements and Habits of Climbing Plants."

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power on which most climbers depend is inherent though undeveloped in almost every plant in the vegetable kingdom."[1]

It will be interesting to try to analyze the conditions under which he made this definite prediction. In his work on climbing plants he showed that the power to climb depends on two quite distinct powers: (1) the power of spontaneous circumnutation, and (2) sensitiveness to touch, and the consequent bending toward the side touched.

Without the theory of descent, the question of the origin of the above mentioned sensitiveness could never have arisen at all. With the theory of descent, and with natural selection as a cause, and a belief in the existence of variations for it to work upon, it might have been possible to infer some general power or tendency in plants as the source of that sensitiveness to touch; but it was not done, and from what has been said elsewhere it is not likely that it would have been done, at least without great difficulty, from a knowledge of the highly specialized effects. Darwin said, "If we inquire how the petiole of a leaf, or the peduncle of a flower, or a branch, first becomes sensitive, and acquires the power of bending toward the

[1] Journal, p. 117.

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touched side, we get no certain answer. Nevertheless, an observation by Hofmeister well deserves attention, namely, that the shoots and leaves of all plants, whilst young, move after being shaken; and it is almost invariably young petioles and young tendrils, whether formed of modified leaves or flower-peduncles, which move on being touched; so that it would appear as if these plants had utilized and perfected a widely distributed and incipient capacity, which capacity, as far as we can see, is of no service to ordinary plants."[1]

Darwin was in search of a source of the sensitiveness of plants, and Hofmeister had provided it by empirical observation. Darwin's relation to this explanation was exactly the same as it was in the discovery of the principle of natural selection. It will be seen that in the latter case he had studied very carefully the effects (adaptations) to be accounted for, and variations as the material upon which the unknown cause might act; then by accidental reading of Malthus the cause was presented to him, and he brought it and its effects into relation with each other by interpreting the latter as results of the action of the former. In the case of the sensitiveness to touch in plants,

[1] Journal, p. 112.

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he was in possession of the cause (natural selection), and the specialized effects and the belief that these are the results of the action of natural selection upon variations; Hofmeister furnished the material which natural selection could develop, by the observation that all young plants are slightly sensitive to disturbance.

Now what was the logical setting for Darwin's prediction that the capacity of *revolving* would be found inherent, though undeveloped in almost every plant in the vegetable kingdom? If there were no evidence to the contrary, it would be supposed that he had been able to make the prediction from the knowledge that climbing plants occur throughout the plant kingdom, and that therefore the source of the variation must be a general tendency in plants. Such an inference seems easy enough to make, as has been shown in other cases, after it has been made. In this case Darwin had the help of an analogy upon which he could depend with confidence. Hofmeister had furnished, by observation, a general source of the sensitiveness of climbing plants in the slight sensitiveness of young leaves and shoots in general. Sensitiveness to touch and power of circumnutation are inseparable in climbing plants; what was more natural, therefore, than the inference that the

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power to revolve had its source in an unknown general tendency, just as sensitiveness to touch had its source in a known slight general sensitiveness, each power having been developed and specialized in climbing plants by natural selection? Such an inference would seem almost inevitable. But in addition he was in actual possession of a case in which the power of revolving was imperfect and functionless.

He said that when he made the prediction he "knew of only one imperfect case, namely, of the young flower-peduncles of a *Maurandia*, which revolved slightly and irregularly, like the stems of twining plants, but without making any use of the habit."[1] In the discussion of *Maurandia semperflorens*, in his original paper on climbing plants, he gave the following interesting bit of history: "I should not have noticed the present species, had it not been for the following unique case. Mohl says that the flower-peduncles, as well as the petioles, are wound into tendrils." Darwin proved that the flower-peduncles do not act as tendrils; but that they nevertheless, whilst young, exhibit feeble revolving powers, and are slightly sensitive to a touch. He observed nine vigorous plants, and it was certain to him that neither

[1] Origin of Species, p. 197.

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the slight spontaneous movements nor the slight sensitiveness of the flower-peduncles were of any service to the plants in climbing. He gave reasons for believing that these imperfect powers are not relics of former functional power, and that correlation of growth did not transfer them imperfectly from the internodes and young petioles to the flower-peduncles; and said that, by whatever means acquired, the case was interesting to him because these useless capacities, by being a little perfected, would make the flower-peduncles of this plant as useful for climbing as are those of *Vitis* and other plants.[1]

The important part which the case of *Maurandia* played in the reasoning is shown by what he said concerning it. What he had said of the source of sensitiveness to touch he almost literally repeated of the power of circumnutation: "If we further inquire how the stems of petioles, tendrils, and flower-peduncles of climbing plants first acquire their power of spontaneously revolving, or, to speak more accurately, of successively bending to all points of the compass, we are again silenced, or at most can only remark that the power of movement, both spontaneous and from various stimuli, is far

[1] Journal of the Linnean Society, 1865; Botany, Vol. IX. pp. 38–40.

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more common with plants, as we shall presently see, than is generally supposed to be the case by those who have not attended to the subject. There is, however, the one remarkable case of the *Maurandia semperflorens*, in which the young flower-peduncles spontaneously revolve in very small circles, and bend themselves when gently rubbed to the touched side; yet this plant profits in no way by these two feebly developed powers. A rigorous examination of other young plants would probably show some slight spontaneous movements in the peduncles and petioles, as well as that sensitiveness to shaking observed by Hofmeister. We see, at least in the *Maurandia*, a plant which might, by a little augmentation of qualities which it already possesses, come first to grasp a support by the flower-peduncles, as with *Vitis* and *Cardiospermum*, and then by the abortion of some of its flowers acquire perfect tendrils."[1] At this point he made the prediction already quoted.

To sum up, Darwin based his conclusions concerning the source of the power of revolving upon the following data: (1) detailed knowledge of the nature and extent of the climbing power in the plant kingdom; (2) proof that there are

[1] Journal, p. 113.

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many gradations of structure and function between simple twiners and tendril bearers; (3) the conviction that all such highly specialized functions and structures are extreme developments of more general but less obvious phenomena, or of slight variations whose source is unknown; (4) the almost perfectly analogous case of sensitiveness to touch in climbing plants, which is inseparably connected with the power of revolving, and which he had connected with the general though slight sensitiveness of plants observed by Hofmeister; (5) and one of the most important of the data mentioned, the actual case of imperfect and functionless power of revolving in *Maurandia semperflorens*, which might by a little augmentation become useful in climbing.

The final verification of the prediction is embodied in the volume on the "Power of Movement in Plants," by Charles and Francis Darwin. The sweeping character of the verification cannot be better indicated than by quoting their own statement of what they intended to show in that volume; it will serve, too, as a summary of results, for they completely established what they claimed: "In the course of the present volume it will be shown that apparently every growing part of every plant is

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continually circumnutating, though often on a small scale. Even the stems of seedlings before they have broken through the ground, as well as the buried radicles, circumnutate as far as the pressure of the surrounding earth permits. In this universally present movement we have the basis or groundwork for the acquirement, according to the requirements of the plant, of the most diversified movements. Thus the great sweeps made by the stems of twining plants and by the tendrils of other climbers result from a mere increase in the amplitude of the ordinary movements of circumnutation. The position which young leaves and other organs ultimately assume is acquired by the circumnutating movement being increased in some one direction. The leaves of various plants are said to sleep at night, and it will be seen that their blades then assume a vertical position through modified circumnutation, in order to protect their upper surfaces from being chilled through radiation. The movements of various organs to the light, which are so general throughout the vegetable kingdom, and occasionally from the light, or transversely with respect to it, are all modified forms of circumnutation; as, again, are the equally prevalent movements of stems, etc. towards the zenith, and of roots towards the

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centre of the earth. In accordance with these conclusions, a considerable difficulty in the way of evolution is in part removed, for it might have been asked, How did all their diversified movements for the most different purposes first arise? As the case stands, we know that there is always movement in progress, and its amplitude or direction, or both, have only to be modified for the good of the plant in relation with internal or external stimuli." [1]

Thus the great work of observation and reasoning began with an effort to explain the power of climbing among plants under the theories of descent and natural selection; passed on to the prediction of the universal movement of circumnutation and its verification; and closed by explaining all the other highly specialized and remarkable movements of plants and plant organs as modifications of the same general but unapparent movement. The principal difficulty at first was the fact that climbers were found throughout the plant kingdom, and could not have been descended from a common climbing ancestor. By the investigations of Darwin and his son, not only were the different methods of climbing shown to be modifications of the twining movements of the stem, but it and all the

[1] Power of Movement in Plants, p. 4.

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other movements of plants were shown to be modifications of a universal movement. What was at first a difficulty in the way of evolution became, like the structure of the flowers of orchids, the ocelli of the peacock, and the expression of the emotions, one of the strongest supports of the theory.

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XII.

UNVERIFIED DEDUCTIONS.

EVERY apparently insignificant fact was full of meaning to Darwin; and he made it the occasion for what he used to call "fool's experiments." His speculative powers employed themselves as actively and energetically on the details of his investigations as on their larger outlines; but he was as ruthless in testing and rejecting his speculations as he was facile in making them. When, however, he had once established a principle, he followed out the deductions from it with as much confidence as if he had already secretly seen the facts whose existence he suspected or thought probable. It is important to note the caution with which he usually stated his anticipations, and to contrast with it the energy and confidence with which he sought and worked out the facts. He spent his life establishing the consequences of his theories, but with all his fidelity and persistence he had to leave many things unproved; some for lack of time, others

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because of the inaccessibility of the facts. He had the satisfaction of living to see the whole biological world applying itself to the work of bringing out the consequences of his theories. It is not a part of the present purpose, even if it were possible, to follow out the logical history of the subsequent work based on those theories. But it will be of interest to notice a few instances in which he made deductions which he could not verify. Some of these have been since verified by others, and some still remain unverified. They vary all the way from confident predictions to vague expressions of a wish that some one would make observations that he thought would bear fruit.

There is one instance of a difficulty in the way of Darwin's theories which is of especial importance. He outlined a possible explanation, but the difficulty has proved itself so stubborn that even some of his adherents feel that his theories could not face many of the same kind. The problem is almost exactly similar to that of climbing plants, and its interest is increased by some recent work that has been done towards its solution.

Electric organs occur in various unrelated species of fishes, and differ so widely in their position in the body, their mode of innervation,

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etc., that they could not possibly have been derived from a single primitive electric organ. In this respect the difficulty was exactly similar to that of the climbing power in plants. After pointing out that the electric organs of the different electric fishes are not homologous, — that they occupy different parts of the body, are differently innervated, etc., — Darwin said that the problem which remains is "by what graduated steps these organs have been developed in each separate group of fishes." In the case of climbing plants he had been able to show many gradations of structure and function, — that even when not fully developed the power to climb was serviceable to plants. He said, "The electric organs of fishes offer another case of special difficulty; for it is impossible to conceive by what steps these wondrous organs have been produced. But this is not surprising, for we do not even know of what use they are." [1]

Darwin mentioned, however, as factors for the solution of the problem, the great differences in the strength of the shocks, the close analogy, as he called it, between the electric organs and muscular tissue, the electrical phenomena of ordinary muscle; and called attention to our ignorance of the habits and structure

[1] Origin of Species, pp. 150, 151.

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of the progenitors of electric fishes, and added, "It would be extremely bold to maintain that no serviceable transitions are possible by which these organs might have been gradually developed." Nevertheless, the subject was unapproachable for him; and his opponents have used the electric fishes as one of the greatest stumbling-blocks in the way of natural selection. Even Romanes felt that the electric fishes present so serious an obstacle that if there were many such he would have to hold in abeyance his belief in the theory.[1] Darwin believed that the facts, because they could not be explained, did not therefore militate against the theory. The much greater difficulty of explaining the case of the electric organs compared with that of climbing plants is due to two important facts. In the latter there were at least some gradations of structure and function known; and even when both were imperfect, it could be shown that they were useful, so that natural selection could act upon them. But there were no imperfectly developed electric organs in fishes which could be appealed to as the source from which the perfect organs might be developed; and what was still more important, in some of the electric fishes the organs,

[1] Romanes, Darwin and After Darwin.

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though perfect in structure, were apparently functionless; they were not known to give off any electrical discharge.

Darwin left the problem unsolved except for the suggestions he offered, and it still remains unsolved in part. Among the reasons for this state of the subject are these: that the habits of the electric fishes have been but little known, that the powers of the electric organs are but little understood, and that both are very difficult to investigate.

The present state of the subject is made interesting by the recent studies of Professor Fritsch.[1] From the first the electric organs have been regarded as modified muscular tissue. They occupy the place of what in other fishes is common muscle. The Pacinian law of the direction of the current in electric organs is that at the instant of the shock the side of the electric plate on which the nerve enters is negative, and the opposite side positive; and in this important respect such of them as are of undoubted muscular origin agree with the common muscular tissue and its electrical phenomena. Fritsch's interesting recent studies on

[1] Gustav Fritsch, Archiv für Anatomie und Physiologie, Supplement Band, 1892; Nature, Jan. 19, 1893, Vol. XLVII. p. 271.

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the *Mormyridæ* of the Nile have removed the objection based on the existence of structurally perfect electric organs which do not give off electric discharges. He showed that these delicate little fishes, hitherto regarded as pseudo-electric, are really capable of giving off shocks that can be felt by the hand and can be easily measured with instruments. He has also shown that in these fishes a part of the organ is in a state of transition from muscular to electric tissue. At the posterior end the electric organ is sharply set off from the muscular tissue; but at the anterior end it graduates more or less perfectly into the muscular tissue, and the organ seems to be actually developing in that direction.

A special difficulty lay in the fact that in *Malapterurus*, an African fish, in which the electric organ lies beneath the skin and encases the body like a sheath, the electric current does not obey the Pacinian law, but takes the opposite direction. Fritsch removed the whole difficulty by giving reasons for the belief that in this case the organ is not of muscular but of cutaneous origin, and represents the cutaneous glands that are so plentiful in some parts of the body. Such an exception, without the explanation of Fritsch, would confuse the whole

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problem of the relation between muscle and the electric organs. With the explanation, it furnishes a good example of how investigation dissolves difficulties.

It is a common saying that the solution of one problem leads to new and frequently more difficult ones. It may almost be said an investigation should be viewed with suspicion if it does not leave more new problems than it started out to solve. The belief of Fritsch that the electric organs of *Malapterurus* are homologous with the cutaneous glands opened up a new phase of the general problem of the origin of electric organs. Another difficulty presented itself to Fritsch in the process of solving the original problem which he set before himself. In his study of the innervation of the electric organs of *Mormyrus* he found that the nerves which supply the electric organs decussate, or cross from one side of the body to the other, after leaving the spinal cord as anterior roots, — much as the optic nerves form the optic chiasma within the brain-case. There is no similar case known of nerve fibres crossing from one side of the body to the other after they have left the central nervous system; and Fritsch properly thinks it to be more difficult to explain by gradual variation and natural

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selection than the origin of the electric organs themselves. He has suggested that this remarkable peculiarity has been developed to insure perfect co-ordination between the electric organs of the two sides of the body, without which there could not be perfect unity of action in the electric discharge, whereas while the tissue is merely muscular it is important that the organs of the two sides should act independently of each other.

Fritsch has also called attention to the fact that the sluggish powerfully electric fishes are carnivorous, and the active feebly electric species are at least mostly herbivorous.

More recently still, Professor Engelmann,[1] of Utrecht, in a histological study of the electric organs in embryos of *Raja clavata*, etc., has established in detail the genetic relations between some of the elements of cross-striped muscle-fibre and the lamellæ of the so called meandrine or striped layer which, lying beneath the nerve end-plate in most species of *Raja*, forms one of the principal constituents of the electric organs in the tail of these fishes. Hitherto little more had been known than that

[1] Th. W. Engelmann, Archiv für die Gesammte Physiologie des Menschen und der Thiere, Band LVII. pp. 149–180, June 16, 1894.

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such relations existed. The whole problem has advanced much farther toward a solution on the morphological than on the physiological side; but, as might have been expected, the demonstration of the exact relations between the various elements of muscle-fibre and those of the electric organs has enabled Engelmann to indicate some very important and fruitful lines for physiological experiment.

Although the homology of cross-striped muscle-fibre and the constituents of the electric organ has been proved even to many histological details, and the morphological changes by which the former has been transformed into the latter have become quite clear, the difficulty of explaining the development of electric organs from muscular tissue by means of natural selection is as great as ever. Engelmann has significantly suggested the existence of functions that we are entirely ignorant of; and refers, by way of illustration, to the recent remarkable discoveries of the effects of the removal, partial and complete, and grafting of the thyroid gland,[1] and to Brown-Séquard's demonstration of the existence of an extremely important "internal secretion" in the renal and

[1] Archives de Physiologie Normale et Pathologique, 1890-1894, and elsewhere.

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other glands.[1] He has marked out a new field of physiological research on the problem of electric organs by the suggestion that the marine biological laboratories be taken advantage of to study the effects which the partial or total removal, or destruction, transplantation, etc. of those organs in various stages of ontogenetic and phylogenetic development will have on the normal functions of the animal. He believes that it might thus be quickly shown that these so called useless organs do serve an important function in the animal economy, and that it might be made clear what that function is; or that it might at least be shown what injury their absence entails upon the animal.

It must be remembered that the problem of electric organs has not yet been attacked in any such way as that in which Darwin attacked the problem of climbing plants. Fritsch by two brief visits brought to light many important facts; on the morphological side contributions are steadily made, but the knowledge on the subject up to date has been brought out piecemeal. Before the mode of development of electric organs can be fully understood, there will have to be not only knowledge of the electrical

[1] Ibid., Series V., Vol. V. pp. 778–786, October, 1893, and elsewhere.

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phenomena of muscle in general, but a thorough investigation of the normal electrical phenomena of fish-muscle, and of the efficacy of slight shocks in water; the habits and environments of the electric fishes, their enemies and their prey, need to be carefully studied. When the investigation is set going on a large enough scale, and along all the lines on which there is at present little more than dense ignorance, there will be in the minds of those who have attended closely to the principal biological investigations of recent years no doubt about the outcome. The logical processes involved in the solution of such problems under the influence of a general theory are practically the same, whether the work is done by one man or by a number of men who attack the problems simultaneously or in succession.

The unverified deduction in the following example is interesting on several accounts. (1) The prediction involved what seemed to many entomologists an improbability. (2) The prediction has not been verified, but the improbability has been removed. (3) It has been often quoted even by naturalists as a case of verified prediction. *Angræcum sesquipedale*, an orchid native to Madagascar, has a long whip-like nectary. On several flowers Darwin

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found it eleven and a half inches long, with only an inch and a half of nectar at the bottom. He proved to his own satisfaction, by a study of the structure of the flower, that it is fertilized by moths that thrust their proboscides and heads down into the flower to the utmost, and declared that moths with proboscides long enough to reach the nectar must exist in Madagascar. "This belief of mine," he said, "has been ridiculed by some entomologists; but we now know from Fritz Müller that there is a sphinx moth in South Brazil which has a proboscis of nearly sufficient length, for when dried it was between ten and eleven inches long. When not protruded, it is coiled up into a spiral of at least twenty windings." The moth with the long proboscis has not yet been discovered in Madagascar; but the fact that there are moths elsewhere with proboscides as long as the one required in the case of *Angræcum* has removed the improbability from the prediction. But the point to be borne in mind is this, that the firm conviction that orchids are fertilized by insects, and that the flowers and insects are co-adapted in structure, led Darwin to believe firmly in an "improbable thing."[1]

The questions raised in Darwin's mind by

[1] Fertilization of Orchids, pp. 162–166.

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his own theories were innumerable; some of them he answered by the great investigations already mentioned, others he left unanswered for various reasons. Only about two months before his death he pointed out new lines of investigation. Among other things he said that there were many inconspicuous flowers not known to be visited by insects during the day, and the natural inference is that they are self-fertilized. And he pointed out the desirability of finding out whether these flowers are visited at night by the innumerable individuals of the many species of minute moths. If they are not so visited, why do they expand at all? Why are they not cleistogamic? He suggested, as a mode of procedure, smearing the flowers with viscid matter and then looking for insect scales; but gave the caution that it would be necessary to prove that the matter employed was not in itself attractive to insects.[1]

It is a fascinating study to follow out the suggestions that came to him and that he made to others, to note the various degrees of success with which the investigations were made by others, to compare the spirit and methods with which they were made with Darwin's own spirit

[1] Müller, Fertilization of Flowers, Prefatory Note, by Charles Darwin.

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and method. He knew the importance of studying the speech of monkeys in relation to his belief in the descent of man, and expected valuable results from it. "I wish," he wrote to Asa Gray, "some one would keep a lot of the noisiest monkeys, half free, and study their means of communication." [1] Mr. Garner has recently written magazine articles and a book on the subject, and has even visited the ape country in Africa with elaborate arrangements for studying the speech of monkeys in their native haunts. It is too early to forecast results, but this case illustrates well how different phases of Darwin's theories have attracted different types of men, and how caution or the want of it may make or break confidence in the results of their investigations.

[1] Life and Letters, Vol. II. p. 183.

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XIII.

ERRONEOUS DEDUCTION.

CONSIDERED simply as a logical process, deduction is no more interesting in the hands of the modern investigator than it was in the hands of the mediæval schoolman. The scientist uses it, apart from its importance in proof, or effort to convince others, merely as an instrument with which to test what is known, and to develop its unknown consequences. The infallibility of the process is altogether hypothetical. The conclusion is true only if the premises are true; and since the truth of the premises is oftener the matter in question than even the investigator dreams, the effort to get at new truth by anticipating the consequences of theory often results in false conclusions. This must especially be the case when there is no apparent reason to question the truth of the premises; when they would seem to have been permanently established by repeated crucial tests. In a number of instances Darwin went

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clearly wrong in his deductions. Some of them he corrected himself, for some he accepted the corrections of others; and some have never yet been corrected directly, but only by the adoption of the contrary principle from a study of facts similar to those on which Darwin went wrong. When his attention had become fixed upon the cowslip (*Primula veris*), he found that there were two forms of flowers on plants of this species. He said, "The first idea which naturally occurred to me was, that this species was tending towards a dicœcious condition; that the long-styled plants, with their longer pistils, rougher stigmas, and smaller pollen grains, were more feminine in nature, and would produce more seed; that the short-styled plants, with their shorter pistils, longer stamens, and larger pollen grains, were more masculine in nature."[1] Nothing would seem more natural than that the structural differences between the flowers should indicate differences in sexual function. The knowledge of dicœcious plants and belief in the modification of species could plainly suggest but one interpretation of the structural differences of the two kinds of flowers. Either they indicated what was inferred from them, or they could

[1] Different Forms of Flowers on Plants of the same Species, pp. 18–21.

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indicate nothing at all. Had his conclusion been left at this point, it would probably have been accepted as both interesting and quite certain. The degree of certitude with which such an inference is received depends on the number of facts involved, their relation to each other, and the degree to which they act as *convergent* evidence toward the one conclusion. In these respects the facts were all that could be desired. It would seem that, if it is possible to make any inferences at all concerning function from the structure of plants, it would have been so in this case.

For Darwin, as for every true student of nature, deductions exist only to be verified. The indirect evidence from structure he supplemented by experiments on the actual production of seed by the two forms. He might have pointed with pride to the cowslip as a plant in an actual state of transition,—as a fine illustration of his theory. But after describing in detail the differences of structure in the two forms of flower in the cowslip, he said, "The question seems well worthy of careful investigation." He made preliminary experiments which of themselves would have been conclusive, but used them to lay a basis for his much more extensive experiments; they suggested

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methods, furnished cautions, and brought out facts enough to determine the most important directions of investigation. He marked a few plants of each form in his garden, in a field, and in a shady wood, and gathered and weighed the seed. In all the lots the short-styled plants yielded, contrary to his expectation, most seed. He gave tables of results, and added that "by all these standards of comparison the short-styled form is the more fertile." In 1861 he made fuller and fairer trials, and found that the same result also held good for some other species of *Primula*. "Consequently my anticipation that the plants with longer pistils, rougher stigmas, shorter stamens, and smaller pollen grains would prove to be more feminine in nature, is exactly the reverse of the truth."

The facts on which his first inference was based were easy to observe; they corroborated each other in a remarkable way, and harmonized perfectly with what was well known concerning dicœcious plants. There was no reason to suspend judgment, for there were no facts that obtruded themselves as objections to the inference. Now if under such circumstances an inference turns out to be exactly the reverse of the truth, what guaranty is there that a conclusion will be correct in *any* case? The answer

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was given in Darwin's researches. The evidence was not all in. The structural evidence, instead of serving as a basis for a true inference concerning the functions of the organs, was found to be in contradiction with them. Darwin showed that both sets of facts were dependent on a hitherto altogether unknown phenomenon, the differentiation of the flowers of a species into distinct sets with marked structural differences for the purpose of making cross-fertilization almost absolutely certain.

The following case is perhaps as interesting as it could be made, to illustrate the danger of accepting as truth inferences that fall short of demonstration, and only represent a high degree of probability. In all the books that Darwin had consulted *Euonymus Europæus* (spindle-tree) is called hermaphrodite. But he found from an examination of the species that about half of the individuals had both stamens and pistils of normal size, and were therefore hermaphrodite; and that the remaining half had pistils of the normal size, but short stamens with rudimentary anthers without pollen, and were therefore properly females. The ovules were of equal size in the two forms. There could not possibly be any other conclusion from the structure of the flowers than the one he sug-

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gested, — that half of the plants, with perfect flowers, were hermaphrodite, and the other half, with rudimentary stamens and anthers and no pollen, were female. What were the centuries of study on plant structure worth, if it was not safe to accept the conviction that female organs of normal size are present for the bearing of seed? Darwin said, "The most acute botanist, judging only from structure, would never have suspected that some of the bushes were in function exclusively males."[1] As in the previous example, he sought to verify his inference by watching the fruit. He watched thirteen bushes, — eight females and five "hermaphrodites." The females yielded abundant fruit, and a single branch two or three feet long from one of them yielded more than any one whole bush among the "hermaphrodites." The inference from structure was almost completely reversed. The species seemed to be practically dicœcious, with the stamens aborted in the females, but the pistils apparently normal in the individuals that were almost exclusively male in function. He might here again have rested the case, and recorded the spindle-tree as dicœcious.

But hear him: "I now determined to observe more carefully during successive seasons some

[1] Different Forms of Flowers, etc., pp. 287–292.

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bushes growing in another place about a mile distant." He did so, and found some variability among the females in the power of producing fruit and seed, and *great* variability in the "hermaphrodites," the latter never producing as much or as fine seed as the other. At this third stage it was clear that the plants of the spindle-tree are neither part of them female and part hermaphrodite, nor part of them female and the rest, with both sets of organs, practically male in function. The truth lay between the two extremes, the variations in the one or other direction depending even on the character of the season. He said, "This case appears to me very interesting, as showing how gradually an hermaphrodite plant may be converted into a dicœcious one." The final result of the long drawn out investigation was in harmony with his general doctrine of the descent of species, and is an illustration of some of the best evidence that has yet been adduced in its support. To him it was interesting, because it showed how gradually an hermaphrodite plant may be converted into a dicœcious one. To the student of scientific method it is interesting as an example of how an investigation, by stopping short of exhaustion of the field, may lead, not only to imperfect, but to false conclusions.

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In the work on the Fertilization of Orchids Darwin's sagacity found full play in the interpretation of the structure of orchids in accordance with the principle of adaptation for cross-fertilization. Confident that all the marvellously complex structures of orchidaceous flowers were adaptations for cross-fertilization, he regarded each case only as presenting a question with regard to the particular mode of the mutual action of the insect and the flower organs. He was frequently able to predict this mutual action in the case of particular species. He never for a moment lost faith in the principle; but in at least one case he went wrong in his interpretation by not applying the principle rigorously enough.

In the *Cypripedium*, or Lady's Slipper, there are two small orifices near the anthers and one large opening in the labellum or odd petal.[1] After discussing the structure of the flower he said, "Formerly I supposed that insects alighted on the labellum and inserted their proboscides through either of the small orifices close to the anthers" to extract the nectar. This inference seemed plausible enough; the small orifices seemed well adapted to accomplish cross-fertilization because they were close to the anthers,

[1] Fertilization of Orchids, pp. 229–231.

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and an insect would inevitably remove the pollinia if it inserted its head into one of them. But he had not clung closely enough to his general principle. Darwin himself accepted the correction of his inference as follows: "Delpino, with much sagacity, foresaw that some insect would be discovered" to remove the pollinia by entering the labellum by its large opening, and crawling *out* through one of the orifices near the anthers; "for he argued that if an insect were to insert its proboscis, as I had supposed, from the outside through one of the small orifices, the stigma would be liable to be fertilized by the plant's own pollen; and in this he did not believe, from having confidence in what I have often insisted on, — namely, that all the contrivances for fertilization are arranged so that the stigma shall receive pollen from a distinct flower or plant. But these speculations are now superfluous; for, owing to the admirable observations of Dr. H. Müller, we know that *Cypripedium calceolus*, in a state of nature, is fertilized in the manner just described."

Darwin's error in this case consisted in not considering one of the important elements of his principle of adaptation for cross-fertilization, — namely, the importance, not only of

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favoring cross-fertilization, but of protecting the stigma against self-fertilization. When this is considered, the *direction* in which the insect entered would appear much more important than comparative ease in getting at the nectar or proximity of the orifices to the stamens.

One of the most remarkable cases of oversight in Darwin's work occurred in connection with the Venus' Fly-trap. This plant is restricted to a very narrowly limited locality in North Carolina, and its organs are highly differentiated for the purpose of catching insects. Darwin could not reconcile the very limited range of so highly specialized an insectivorous plant with his general belief that the best adapted plants and animals spread over the earth and survive. In his discussion of *Dionæa*, (Venus' Fly-trap), he said, "It is a strange fact that *Dionæa*, which is one of the most beautifully adapted plants in the vegetable kingdom, should apparently be on the high road to extinction. This is all the more strange as the organs of *Dionæa* are more highly differentiated than those of *Drosera*." [1]

All the studies of forty years had borne out the conviction that the adaptations of animals and plants to their environment were the result

[1] Insectivorous Plants, p. 358.

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of natural selection acting upon variations, and preserving those that conduced to the preservation of the species. Following this principle, it seemed almost self-evident that the more highly specialized the organs of a plant or animal are, and the more minutely adapted to its surroundings and mode of life they become, the more certain would the species be of continued existence and of success in the race of life. He never, so far as I know, recognized the inevitable consequence of extreme specialization. Had he pursued the deduction to the end, he must have recognized the fact that a high degree of specialization for a particular mode of life is the mess of pottage for which the birthright of the species has been surrendered.

Darwin was familiar with, and recognized the value of Agassiz's generalization that the progenitors of the greater animal and plant groups have been generalized forms. He himself deduced from his general theory the principle that the species of the larger genera, the wide ranging, much diffused, and common species vary most, that they are more closely related to each other, and that in this respect they more nearly resemble varieties than do the species of the smaller genera with restricted

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distribution.[1] It is clear that the wide range of a species, instead of depending on a high degree of specialization for any one environment, depends rather on the absence of it. He clearly recognized and pointed out the danger of extinction to a species of limited range, but nowhere recognized explicitly the connection, on the one hand, between a high degree of specialization for a particular environment or mode of life and restriction of the species to that particular environment, or the relation, on the other hand, between wide range over the earth and generalized characters which give some *general* advantage that would be useful under all or nearly all circumstances into which the species might be thrown.

He said: "If we ask ourselves why this or that species is rare, we answer that something is unfavorable in its conditions of life; but what that something is we can hardly ever tell." He insisted that the improved and modified forms would crowd out and exterminate the less well adapted forms; but did not hit upon the truth that, the more beautifully adapted a species is to a definite locality and set of conditions, the less it is adapted to enter into a general competition for the possession of the

[1] Origin of Species, pp. 42–44.

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earth. Both Agassiz's empirical generalization that the progenitors of the principal plant and animal groups were generalized forms, and the deductive consequences of Darwin's own theory of natural selection would indicate that the highly differentiated forms are forever handicapped. They have a present advantage in having intensified their adaptation to a certain environment; but in the *long run* they are doomed because they have lost the power of adaptability to new conditions in direct proportion to their present gain.

It is curious that Darwin had before him, and mentioned in the same sentence, a case of each kind: *Dionæa*, with its extreme adaptations for insect catching, with only a single species in the genus, restricted to a very small locality, on the verge of extinction; and *Drosera*, with the same *general* advantage of catching insects, but with no extreme adaptations, with a vast number of species in the genus, distributed all but everywhere over the earth. It is as great a surprise that he did not see the connection between extreme specialization and extinction on the one hand, and general advantage and wide distribution on the other, as he says it was to him that *Dionæa* is on the verge of extinction. It is clearly inferable from

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these cases and the general principle of natural selection that Nature has pronounced the sentence of death upon highly specialized forms; that they have passed out of the royal line of descent for a special advantage; that if they vary at all it can only be within the restricted lines along which they have already gone so far; and that the birthright belongs to forms which have some general advantage, but are not hampered by special adaptations. These spread over the earth and from *them* branch off the numerous closely related and variable species that occupy all the environments of the earth. Thus *Drosera*, with its power to catch insects, is yet a plant like others, and has gone forth with its advantage to possess the earth. *Vitis*, with its climbing power, has scattered its species by the hundred over the earth. It is the modification that opens up to the species a large area, which makes it possible for the species to send out its kind into the whole earth to be everywhere modified by local influences.

The principle that has been discussed is now one of the well understood corollaries of the principle of natural selection. I do not know who first called attention to it. It has probably occurred to many minds independently.

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Darwin, so far as I know, never recognized it. The earliest clear statement of it that I have seen is by Prof. Joseph Le Conte in an article on "Instinct and Intelligence,"[1] published in 1875, only a few months after the publication of Darwin's "Insectivorous Plants." He said, "Instinct, therefore, is accumulated experience, or knowledge of many generations fixed permanently and petrified in brain-structure. All such petrifaction arrests development, because unadaptable to new conditions. They are found, therefore, only in classes and families *widely differentiated from the main stem of evolution*, from the lowest animals to man. Instincts are, indeed, the flower and fruit at the end of these widely differentiated branches, but flowering and fruiting arrest onward growth." In 1877, Marsh, in a discussion of the suilline type, stated the true principle when he said that ambitious offshoots have perished, while the generalized or rather unspecialized forms continue the line of life with true suilline stubbornness.[2]

[1] Popular Science Monthly, October, 1875, p. 664.

[2] American Journal of Science, XIV. (III.), pp. 362–364.

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XIV.

GENERAL DISCUSSIONS.

DARWIN'S general discussions of the various subjects that he worked out in such minute detail are models both of clearness and of exhaustiveness. The reasoning of the second volume of the "Variation of Animals and Plants under Domestication" is long sustained and characterized by the use of enormous numbers of facts. The latter is one of the chief characteristics of his reasoning. He was frequently compelled, as in the case of the "Origin of Species," by the limits within which he was obliged to condense his materials, to substitute general statements for long series of facts. The generalizations were condensations of the detailed facts, which were too bulky for his pages. This is the reason why his discussions leave the impression of an almost infinite reserve of evidence; and rightly seem to convey much more to the reader than is actually written on the page.

Among Darwin's many exhaustive discus-

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sions of the materials which he had collected, some of the most striking are his chapters on pigeons,[1] in which he considered the variation of breeds, individual differences, and skeletal differences; the discussion in the second volume of the "Variation of Animals and Plants under Domestication"; the exhaustive analysis of his materials in reaching his general conclusions on the effects of cross- and self-fertilization; and his repeated discussions of the cement glands of Cirripedia, and of the parasitic and complemental males on the hermaphrodite "females," and the reasons for regarding them as such rather than as independent forms.[2] But the general discussion that is typical both from the general interest of the subject and the compactness and symmetry of the argument is the work on the "Origin of Species." It would be impossible to analyze such a far-reaching argument without restating it. Doubtless criticisms could be made against the arrangement of materials and the order of discussion, and against the nature of the evidence adduced. It is not intended here to raise the

[1] Variation of Animals and Plants under Domestication, Vol. I. pp. 137–235.

[2] Monograph of Cirripedia, Vol. I. pp. 38, 180–293, Vol. II. pp. 23–30, 151.

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question whether his argument as a whole is sound and carries conviction with it, but simply to repeat that, considering the materials that Darwin had to work with and the difficulties under which he labored, the argument is finished, and will always serve as a type of probable proof.

Darwin himself has given the reasons for this state of his great discussion. He has complained that he must be a very slow thinker; and doubtless the truth in this complaint accounts for the fact that his thinking was always so thorough. He also bewailed the fact that he experienced great difficulty in writing.[1] He felt that he had great ability to get things wrong end foremost in his expression, and said that he spent a great deal of time in *arranging* the matter in his larger works. From one point of view it is a paradox that such a man should accomplish so much that has proved of permanent value. But the lack of natural felicity of expression and inability to think rapidly, together with his persistence, insured him against the vice of saying things nimbly, and furnished the guaranty that whatever he did would be thorough. It is safe to say that a far larger proportion of false and inaccurate

[1] Life and Letters, Vol. I. p. 80.

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statements and arguments with fatal flaws in them are made by writers who express themselves easily, than by writers whose rhetorical inability compels them to be painstaking. When the expression is laboriously evolved by an intellect that is otherwise strong, the thought comes during the process to be regarded from more points of view. In details there is greater assurance of accuracy, and the proper relative importance is more likely to be assigned to the different phases of the truth. Given two intellects equally conscientious, the slower moving and more deliberate one will always hit upon more phases of the truth than the quicker one.

Darwin attributed the success of the "Origin of Species" to the way in which it was developed. Only when he had spent a number of years in investigation, after he had got hold of the theory of natural selection, did he allow himself, in 1842, to draw up a brief thirty-five page sketch of it. In 1844 he wrote a larger sketch of two hundred and thirty pages. Then followed years of laborious investigation, the vast results of which were cast into an abstract which, if it had been published, would have been a very large work. Upon the urgent advice of his friends, Lyell and Hooker, he decided not to delay publication any longer;

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but the large work was not in condition for publication. An abstract of it was made; and this abstract was the "Origin of Species." He had written two condensed sketches, and finally abstracted a much larger manuscript which was itself an abstract.[1] Probably no work was ever better tempered and tested before it reached the public eye.

[1] Life and Letters, Vol. I. p. 70.

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XV.

LOGICAL HISTORY OF THE PRINCIPLE OF NATURAL SELECTION.

DARWIN'S own views of method, his treatment of evidence, and some of the various logical processes which he employed in his investigations, having been discussed and illustrated by examples from his many works, it now remains necessary to trace the logical history and examine the present logical status of the principle of natural selection, which gave inspiration to and lay at the basis of his life-work. The theory of natural selection has permeated and colored modern thought more deeply than perhaps any other scientific theory, and this fact alone makes the study of its logical history extremely interesting.

The scientific influences, both in the form of teachers and of books, to which Darwin was exposed during and after his university life, were opposed to the already well known doctrine of the descent of species. Various reasons have been assigned for the failure of the doc-

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trine to impress itself upon scientific men. The difficulty did not lie in the circumstance that the facts of botany and zoology were opposed to it; for it first took its rise out of them. The affinities of species and of the higher groups, and the facts of embryology, distribution, and palæontology by themselves were sufficient to force the conviction that species are derived, and the doctrine would doubtless have won its way at once had it not had to make head against the imported belief in creation. Had the doctrines of descent and creation been for the first time presented to the scientific mind as alternative beliefs, there can be no doubt that the former would have been chosen as the true explanation of the facts, even though no force capable of producing the effects had been assigned. The cause would still remain to be investigated, while the facts would be brought together under a single point of view. With the adoption of creation as an explanation, an efficient cause is provided, but the facts remain worthless either to prove or to disprove the doctrine. It is not enough that a cause should be capable of producing given effects, but it should produce the given effects and be incapable of producing any other set of effects. In short, by the former view the

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special character of the facts is accounted for, but from the latter it is impossible to deduce the specific character of any phenomena. Any other set of facts exhibiting a plan or purpose of any kind could be deduced with equal ease from the doctrine of creation.

When Darwin started on the Beagle voyage he was orthodox on the question of the origin of species. As he travelled, and as his knowledge of zoology and palæontology became wider and deeper, the doctrine of descent began to take hold of him. The relation of the living animals to the fossil species in South America, the manner in which closely allied animals replaced one another as he proceeded southward over the continent, the South American character of the productions of the Galapagos archipelago, and especially the slight but distinct differences of the flora and fauna on neighboring islands of the archipelago, impressed him so strongly with the peculiar character of the facts and the necessity of a *definite* mode of origin that he began to see the difference in the logical characters of the doctrines of creation and descent.[1] The facts were better explained by the latter than by the former; and he connected them at least tentatively with the

[1] Life and Letters, Vol. I. p. 67; Origin of Species, p. 2.

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old doctrine of descent. But as he himself remarked, a naturalist might be convinced by affinities, embryology, distribution, etc., that species are derived by descent; but the conclusion, though correct, would be unsatisfactory until it was shown *how* it was brought about.

Lamarck had assigned causes for the modification of species; but Darwin insisted positively that he derived no help whatever from him. Logically, the failure of the former's doctrines to win over the scientific world was due to his connecting the doctrine of descent, which had strong evidence in its favor, with hypothetical causes which he did not subject to rigid deductive tests in explanation of the facts. What kept Darwin longest orthodox were the facts of adaptation. No cause could be considered adequate which did not account for the exquisite adaptation to environment found throughout the animal and plant kingdoms. As he said himself, "I had always been much struck by such adaptation, and until these could be explained it seemed to me almost useless to endeavor to prove by indirect evidence that species have been modified."[1] He distinguished clearly in his own mind between the two propositions which he undertook to prove

[1] Life and Letters, Vol. I. p. 67.

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in the "Origin of Species": first, that species are derived by modification from other species, and, secondly, that Natural Selection is the chief cause of this modification. He did not originate the former; the logical relation of his work to it is deductive, and largely took the form of an answer to the question, Do the facts of Nature harmonize with the hypothesis?

The cause of the modification of species could not even be raised as a question until the fact of modification had been accepted, at least tentatively. He recognized the cause of specific modification as a problem to be solved, a principle to be discovered by induction from the effects it produced in the form of adaptations. To quote his own words: "My first note-book was opened in July, 1837. I worked on true Baconian principles, and without any theory collected facts on a wholesale scale, more especially with respect to domesticated productions," etc.[1] He had rejected Lamarck's suggestions, and it would seem impossible to imagine a more interesting or more purely inductive problem than that which presented itself to him. There was for him no clew to the cause which he wished to discover except in the vast wealth of material which he regarded

[1] Life and Letters, Vol. I. p. 68.

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as its effects. To have discovered the cause by an analysis of the effects would indeed have been a triumphant inductive discovery.

He selected wisely the material on which to concentrate the investigation; he said: "I soon perceived that selection was the keystone of man's success in making useful races of animals and plants. But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me." In his study of domestic races he observed both the effects (races) and the cause (selection), and did not, except perhaps in details, reason deductively from the cause to discover the effect, or inductively from the effects to discover the cause. The effort to extend the principle of selection by induction to animals and plants in a state of nature failed because it was impossible to see how the principle could be applied. The inductive problem was apparently as far from solution as at the beginning; he was still groping in the dark. It would be a bootless speculation to try to answer the question whether Darwin could ever have solved by a study of adaptations the problem which he set for himself. It would be hardy to hold that a man with Darwin's intellectual and moral resources, with his clear conception

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of the problem and the data from which it was to be solved, could not have derived the cause from an analysis of the effects; and yet very few problems like this were ever solved by pure induction. It may be possible to infer the nature of a cause from the nature of the effects, but nearly always observers manage to catch a glimpse of the cause at work. Then, by a generalization, the cause is extended to all the other effects of the same kind.

In October, 1838, at the end of fifteen months of work on Baconian principles, without any theory, he read Malthus on Population for amusement.[1] There had been much discussion in the eighteenth century concerning the vice and misery in human society. It was quite commonly believed that they were due to the organization of society, and that they could be eliminated by reorganization of society according to some ideal. The father of Malthus shared this view; but the son, in discussion with him, took the position that, no matter how society might be organized, vice and misery would follow inevitably from the fact that the human race naturally increases more rapidly than the means of subsistence. This notion was finally developed into the "Prin-

[1] Life and Letters, Vol. I. p. 68.

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ciple of Population," which fell into Darwin's hands. The purpose of Malthus in this work was to investigate the causes that had hitherto impeded the progress of mankind toward happiness. After establishing the principle that population has a tendency to increase in geometrical ratio, while the food supply can at best increase only in arithmetical ratio, he pointed out that the ultimate check to the increase of population is lack of food; and that all the immediate checks could be included under three heads, moral restraint, vice, and misery, — and urged moral restraint as a check to population, because by it alone could vice and misery be driven out of the world.[1]

Malthus stated with perfect clearness "the constant tendency in all animated life to increase beyond the nourishment prepared for it," and the consequent struggle for existence; and insisted that in every country, speaking of the human family, some of the checks to population are, with more or less force, in constant operation.[2] He recognized both artificial and natural selection as results of the struggle for existence. In the chapter on the "Checks to Population among the American Indians," he said: —

[1] Malthus, Principle of Population (9th edition), pp. 1–8.

[2] Ibid., p. 9.

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"As the parents are frequently exposed to want themselves, the difficulty of supporting their children becomes at times so great that they are reduced to the necessity of abandoning or destroying them. Deformed children are very generally exposed; and among some of the tribes in South America, the children of mothers who do not bear their labors well experience a similar fate from a fear that the offspring may inherit the weakness of its parent.

"To causes of this nature we must ascribe the remarkable exemption of the Americans from deformities of make. Even when a mother endeavors to rear all her children without distinction, such a proportion of the whole number perishes under the rigorous treatment which must be their lot in the savage state, that probably none of those who labor under any original weakness or infirmity can attain the age of manhood. If they be not cut off as soon as they are born, they cannot long protract their lives under the severe discipline that awaits them. In the Spanish provinces, where the Indians do not lead so laborious a life, and are prevented from destroying their children, great numbers of them are deformed, dwarfish, mutilated, blind, and deaf."[1]

[1] Principle of Population, Chap. IV. pp. 20, 21.

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It seems almost astounding that Malthus did not recognize the importance of this principle of natural selection based on the struggle for existence and develop it deductively. Had he worked out the principle of the survival of the fittest among human beings after he so clearly recognized it, it would have borne rich fruit for the happiness doctrine. It would have removed much of the gloom from his principle of population, by showing that much permanent good — much more, in fact, than from moral restraint — arises from the struggle for existence by its preserving those best fitted to enjoy life. From another point of view, the fact that he did not develop the principle of natural selection, at least within the human race, after he had so plainly recognized both its action and its effects, is not even remarkable. When one has once made a study of the deductive powers of such a man as Darwin, and finds that with his great logical strength he sometimes failed altogether, and was often very long in reaching deductively the consequences of his theory, it is not to be wondered at that Malthus did not grasp one of the most important features of his principle.

Since he failed to apply the principle of natural selection within the human species,

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after he had himself stated clearly its action and results, there is no cause for wonder at his not applying it to the derivation of species. In fact, there was a good reason why he should not do the latter. He expressed himself vigorously in opposition to the belief that a species can vary by an indefinite amount in any given direction; and denied Condorcet's theory of the indefinite perfectibility of the human race.[1] He admitted only a limited amount of variation within a species. Malthus had worked out the struggle for existence and recognized its selective action, at least within a limited range; and as a natural theologian he must have been acquainted with numerous adaptations. But he did not connect them as cause and effect, because he did not admit general variability of species, without which there could be no material for the cause to act upon.

When the work of Malthus fell into Darwin's hands, the latter was in possession of the doctrine of descent, and many facts in harmony with it, and hence the conviction in his mind that there was an efficient cause for these facts. The adaptations found in nature seemed the most difficult facts to explain under the theory of descent, and Darwin was already widely

[1] Principle of Population, p. 270.

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acquainted with these. He had recognized, as did others before him, specific variations as material out of which new species must be made, and had gone to work systematically to study variations, especially in domestic pigeons, the most favorable group of animals that could have been selected; and had recognized selection as the key to man's success in the production of races. His study hitherto had helped him to a wider knowledge and clearer notion of adaptations, — the effects that had to be explained; and a better understanding of the nature and range of variations, — the materials upon which the cause must have acted to produce the effects. And in the case of domestic productions he was in full possession of the cause, the materials on which the cause acted, and the results, in selection, variations, and races. As he himself said, he had been fully prepared by his long study of the habits of animals to appreciate the struggle for existence which everywhere goes on. "It at once struck me," he said, "that under these circumstances favorable variations would be preserved and unfavorable ones destroyed. The result of this would be the formation of new species. Here, then, I had at last got a theory by which to work." [1]

[1] Life and Letters, Vol. I. p. 68.

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It is not recorded at what point in his reading of Malthus it struck him that the struggle for existence, by working upon variations, would produce new species. Probably with his mind so thoroughly imbued with the subject that everything he read was made to bear upon the problem he almost instantaneously caught the significance of the principle. Malthus himself stated the principle clearly in the first few pages of the book, and already on pages 21, 22, of the ninth edition stated its action and effects upon the American Indians. The significance of these details is great from the logical point of view. All the years of the Beagle voyage had prepared Darwin to appreciate the principle. The time since his return had strengthened his belief in the descent of species, and his efforts to reach the cause of modification inductively had brought him detailed knowledge of both variations and adaptations. What he had not hitherto been able to discover by induction came to him by accident, if it can be said that anything can come by accident to a mind so much on the alert for it. It would have been one of the most fascinating chapters in scientific discovery if he had recorded in detail the mental activity and the feelings that must have flooded him from the moment the discovery was

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made. There was an intellectual explosion, a flash of the mind, and from that moment his life-work was devoted to elaborating the consequences of the principle. The facts which he had been gathering and reflecting upon were explained as the effects of the cause which Malthus presented, and gathered a new significance from it.

"Here, then, I had at last got a theory by which to work," he said. The groping was at an end. His future work was outlined. The confession in that sentence can be appreciated only by one who has in his own experience passed from the mental strain and perplexity of a purely inductive effort to the solid ground afforded by even a fairly probable hypothesis. Doubtless his work was thenceforth many times more rapid than it could otherwise have been; for with so vast a number of facts to be considered the theory itself was the only pathfinder. Only after the discovery of the principle could the work of gathering up and classifying known facts and of searching for new ones, of reducing exceptions and apparently unexplainable groups of facts, go on apace. The logical process by which adaptations, variations, and the struggle for existence were brought together into the relation of cause and effect was deductive; and

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the principle of Natural Selection still depends for its logical support upon that power of deductive explanation which Darwin recognized in it the day he read Malthus on Population. It has penetrated every field of thought, but in the field in which it first gathered strength it is still without direct demonstration. It has been made the basis for countless deductive operations, but it leans for support on the very structures thus erected. Writing to Bentham, in 1863, concerning the proofs of natural selection and the descent of species, Darwin said, "Belief in natural selection must at present be grounded entirely on general considerations: (1) on its being a *vera causa* from the struggle for existence; . . . (2) from the analogy of change under domestication by man's selection; (3) and chiefly from this view connecting under an intelligible point of view a host of facts."[1] To Huxley he said, in December, 1860, "I can pretty plainly see that, if my view is ever to be generally adopted, it will be by young men growing up and replacing the old workers, . . . and finding out that they can group facts and search out new lines of investigation better on the notion of descent than on that of creation."[2]

[1] Life and Letters, Vol. II. p. 210. [2] Ibid., p. 147.

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From a logical point of view the work of the last thirty years in the various fields of biology has been a series of deductions and verifications of the original propositions laid down by Darwin. He saw from the beginning that belief in his theory must rest on general considerations, the chief of which was its power to facilitate deductive investigation; and there it still rests. At this late day the chief apostle of natural selection says that it is really difficult to imagine the process of natural selection in its details, and that it is impossible to this day to demonstrate it in any one point.[1] It is the logical relation of the principle to the facts that makes it invaluable in modern thought. The whole logical history of Darwin's principles illustrates what Mill said of the deductive method. "To the Deductive Method thus characterized in its three constituent parts, Induction, Ratiocination, and Verification, the human mind is indebted for its most conspicuous triumphs in the investigation of nature. To it we owe all the theories by which vast and complicated phenomena are embraced under a few simple laws, which, considered as the laws of these great phenomena,

[1] Weissmann, Contemporary Review, September, 1893, Vol. LXIV. p. 322.

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could never have been detected by their direct study." Writing of the celestial motions as an illustration, he continued, "How could we ever have ascertained the combination of forces on which the motions of the earth and planets are dependent by merely comparing the orbits or velocities of different planets or the different velocities and positions of the same planet?"

Darwin himself did not discover the cause by the direct study of the effects; but his efforts to reach a cause inductively gave him such an insight into variations and adaptations that he could prosecute vigorously the other two steps, deduction and verification, when once the cause was given. What Mill said of celestial motions could be almost literally quoted of adaptations. It would hardly be going beyond the facts to say that the history of theories proves that usually not even preliminary hypotheses concerning causes are worked out directly from an analysis of effects; but the causes are usually caught in action during the effort to discover them inductively, or are reached in a roundabout way.

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XVI.

CONCLUSION.

MUCH might be said concerning the personal qualities of the man that did so much scientific work of such uniformly high character. The moral force that overcame life-long physical suffering, that stood through many years of silent toil face to face with the certainty of abuse for its reward, that never knew defeat and remained calm during the years of victory, has a powerful influence on the student of Darwin. The utter lack of partisanship for any idea, the rare judicial temper that made truth seem better than any theory, the penetration, the power of concentration, the firm mental grasp, the inability to leave anything unexplained, — all these high qualities have their silent evidence in the character of Darwin's scientific work. But his intellectual and moral traits have been touched upon here only in so far as was necessary in order to discuss clearly his use of the logical processes. The effectiveness of these processes

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must always be entirely dependent on the character of the individual using them.

Darwin's views on method can be summed up in the assertion that he was afraid of every statement or hypothesis until it was tested, and indeed regarded an unverified belief as worthless. The starting points of his investigations were frequently what seemed to other men interesting, but unimportant or inconvenient exceptional facts. When he sought explanations he seemed to be trying to get a conclusion by the shortest and easiest route, with as little labor as possible. But when he had once got an hypothesis he dragged it before all the multitude of facts that could be made to bear witness to its truth or falsity, until it seemed as if he were trying to make the investigation last as long as possible. Time seemed no longer worth considering. He always used the isolated phenomena which were most difficult to explain as tests of the validity of his hypotheses. By considering all possible objections during the progress of the development of his conceptions, he threw a merciless light on the weaknesses of his theories, and thus gave them, in their final form, as high a degree of probability as was possible. In his treatment of negative evidence he never lost sight

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of its comparatively small value, and continued the investigation until, by careful observation of all collateral facts, he was able to combine them into a body of positive evidence in support of a different or supplemental theory.

Classification was with him an invaluable instrument for extracting information from bodies of facts. His works teem with comparative tables and statements of results derived from them. Analogical reasoning, with all its strength and weakness, was utilized as a powerful instrument of suggestion. Induction was constantly active in the formation of hypotheses. He could leave nothing unexplained. He made an hypothesis for everything, and then tested it unmercifully by deduction. He appreciated the immense importance of theory to good observation, explained in the light of his general theories great bodies of facts and principles which had been discovered empirically, and anticipated many important consequences of those theories. Whenever it was possible he undertook to verify those anticipations; but did not hesitate to make predictions that he could not verify. And with all his vast and accurate knowledge of facts and his logical power, he frequently fell into erroneous reasoning.

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The processes employed in scientific investigations, although some of them have been treated in separate chapters, have a vital interdependence. Darwin did not and could not use one process until its resources were exhausted, and then turn to another. It was the very swiftness with which different processes were successively brought to bear upon his problems that made it possible to digest so thoroughly every set of facts with which he dealt. Whatever may be the future of the particular conclusions which Charles Darwin reached, the general method which he employed and the general drift of his conclusions will have a permanent value. All his efforts tended toward the unification of knowledge. All his inductions became corollaries of one great theory; all his deductions had to do with efforts to test and prove the truth of that theory. The subordination of all the devices of the intellect to one great comprehensive purpose has given a unity of aim to all the great works of his life, has made his general method conspicuously lucid, and has knit the products of his intellect into one great logical whole.

THE END.

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